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The persistence versus flexibility dilemma in executive control

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THE PERSISTENCE VERSUS FLEXIBILITY DILEMMA

IN EXECUTIVE CONTROL

by

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Dedication

Several research mentors have been inspirational in both my professional and personal lives. In chronological order of entry, they are Holly Tuokko, Joe Barrash, Lori Buchanan, Robin Hanks, and Doug Shore. I wish to highlight the contributions of my dissertation advisor, who has taught me much over the years, not just on topics germane to our research, but also with regard to being a good husband, pleasing a discerning palate, traveling in style, avoiding office politics, and everything in between. Mohsan Beg, Joe Casey, Norm Fichtenberg, Brad Hallam, Jeff Martzke, Jeff Wertheimer, and others have also provided wise guidance along the way. My classmates helped to make my graduate school experience tolerable and even pleasant at times. I am especially appreciative of my wonderful wife Gail. Without her subtle glances seeming to indicate that she could not tolerate living in Windsor for much longer (and sometimes far less subtle pleas), I might still be slowly progressing towards a Masters degree. My parents' periodic inquiries about when I would be getting a real job probably also motivated me to cut my rest breaks short. I also want to thank God, whose support I have felt throughout this and all of my endeavors.

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Introduction

An automobile driver must carefully apportion their attention to multiple tasks, such as pressing the accelerator, monitoring the speedometer, adjusting the steering wheel to maintain proper alignment, visually scanning for a highway exit, and formulating a mental list of needed groceries, just to name a few. Some stimuli may "grab" the driver's attention (e.g., the sudden realization that an errand was neglected), for better or worse. Other stimuli may be less compelling but nevertheless warrant consideration at times, requiring the driver to volitionally redirect their attention (e.g., glance at the fuel tank gage). This driving example illustrates several principles of the human attention system. First, the content of attention may be an object in our external environment or internal mental activity (e.g., Gehring, Bryck, Jonides, Albin, & Badre, 2003). Second, our attentional resources are limited (Shapiro, 2001), which necessitates the frequent switching of attention during complex tasks (Burgess, 2000). Indeed, the Oxford English Dictionary definition of multitasking, the "ability to perform concurrent tasks or jobs *by interleaving* [italics added]," acknowledges this reality. Third, whether we maintain or switch the focus of our attention is influenced by bottom-up (stimulus-driven) and top-down (volitionally controlled) forces (Monsell, 2003). The interplay between these forces determines the focus of our attention at any given moment (Yantis, 2000).

Attention switching is seamlessly coordinated, allowing us to (usually) evade such misfortunes as traffic accidents. This is achieved by *executive control*, the ability to volitionally direct attention in mental and physical space. To appreciate the importance of executive control, consider the consequences of its momentary incapacity, such as delaying to disengage from one task and switch to another, or equally problematic, prematurely disengaging because of a distraction. Such failures occur on occasion in healthy individuals, especially when they are fatigued (Manly, Lewis, Robertson, Watson, & Datta, 2002) or when their attention is taxed (Robertson, Manly, Andrade, Baddeley, & Yiend, 1997), and can be markedly exaggerated by brain disease. Certain brain-damaged patients tend to, on one hand, get stuck on an idea or activity, unable to desist. On the other hand, they may be highly distractible such that every noise, movement, or spontaneous thought captures their attention. Executive control is thought to be the most fundamental, yet perplexing problem facing cognitive neuroscientists (Logan, 2003; Monsell, 2003).

Despite its widespread acceptance in the research literature, the term "executive control" is fraught with ambiguity. It misleadingly suggests equivalence with the term *executive functioning*, which refers more generally to all capacities that support independent purposive behavior (Lezak, Howieson, & Loring, 2004). It also falsely implies that a unitary entity (i.e., homunculus) exerts the control. As shall become clear, control emerges from the interaction between complementary cognitive processes (Gruber & Goschke, 2004), not from a mysterious and unmeasurable "puppet-master."

Executive control is closely related to the "anterior attention system" (Stuss, Shallice, Alexander, & Picton, 1995), "supervisory attention system" (Norman & Shallice, 1986), and volitional attention network (Mesulam, 1999), all of which can be distinguished from involuntary attentional systems. The most familiar typology of attention, developed and revised by Posner and colleagues (Berger & Posner, 2000; Posner & Dehaene, 1994; Posner & Peterson, 1990; Posner & Raichle, 1994), differentiates executive control from two other networks responsible for orienting and alerting/vigilance, respectively. Understanding the concept of executive control in the context of other higher-order cognitive functions involved in voluntary action is complicated by a lack of consensus in the field. Executive control is probably necessary but not sufficient to conceive, plan, and carry out novel goal-directed behavior. It can therefore be thought of as a component executive function (Lezak et al., 2004). However, the distinction between executive control and other components, such as working memory and response inhibition, is unclear (Wecker, Kramer, Hallam, & Delis, 2005). Some theorists believe them to be distinct constructs (Logan, 2004; Miyake et al., 2000) that work in harmony (Roberts & Pennington, 1996), whereas others remain unconvinced that they are theoretically separable (Kane, Bleckley, Conway, & Engle, 2001; Kimberg & Farah, 2000).

Much work over the past decade has begun to elucidate the cognitive architecture of executive control. Early models that posited an omnipotent homunculus that directed our attention – as exemplified by Baddeley and Hitch's (1974) "central executive" and Norman and Shallice's (1986) "supervisory attentional system" – have become understood to be placeholders awaiting explanation rather than adequate accounts of executive control. This led to a drive towards abandoning the concept of a unitary attention control center or at least fractionating it into definable components (Baddeley, 1996; Monsell & Driver, 2000; Parkin, 1998; Stuss & Alexander, 2000). Hommel, Daum, and Kluwe (2004) suggest parameters for this vague goal. They argue that an adequate model of executive control must specify *what* is controlled and *how* it is controlled, and ultimately, *who* does the controlling. Accounts for these three components based on an integrated literature review will now be discussed in turn.

"Mental sets" are *what* is controlled. A mental set is a response disposition or state of readiness for action. More formally, it is a schema that biases a cognitive network towards a certain action in response to a given stimulus (Mayr, 2003). These biases are created by constraints that come from multiple sources. One source of constraint is long-term memory (i.e., the network's structure), or stimulus-response mappings that gradually develop over time with practice. For example, a cup (the stimulus) is typically grasped with the dominant hand and drawn to one's lips (the response). Constraints can also be activation-based. These may be internally (e.g., activated goals) or externally (e.g., task instructions) generated. These activation-based constraints modulate the network's structure, thereby biasing it to respond a certain way to a given stimulus.

Explaining how mental sets lead to behavioral responses, Goschke (2003) posits that response selection occurs as a process of constraint satisfaction. A mental set prepares the stage so that once the stimulus is presented, the chain of events leading to a response can then unfold automatically in a "prepared reflex" (Hommel, 2000). William James (1950, c1890) elegantly summarized this view more than a century ago, stating that "the essential achievement of the will, in short, when it is most 'voluntary,' is to attend to a difficult object and hold it fast before the mind. The so-doing is the fiat; and it is a mere physiological incident that when the object is thus attended to, immediate motor consequences should ensue" (pg. 561).

Perhaps the most striking evidence for mental sets and their link to overt behavioral action comes from brain-injured patients who exhibit "utilization behavior"

(Archibald, Mateer, & Kerns, 2001; Lhermitte, 1983), in which actions are elicited by common objects. For example, a patient may drink from a cup, not to achieve an intended goal (e.g., quenching thirst), but because the mere presence of those objects triggers their highly-associated actions.

Multiple (potentially task-relevant) mental sets may be stored in working memory simultaneously, with only one being activated above a certain threshold (Norman & Shallice, 1986). Switching mental sets, then, would require reallocation of attention to another mental set within working memory. This view has been criticized on the grounds that it would be maladaptive to hold multiple sets of rules in working memory simultaneously because they would cause crosstalk, or interference, especially in the case of overlapping stimulus-response mappings (Rubinstein, Meyer, & Evans, 2001). An alternative view is that only the selected mental set is activated in working memory and the others are eliminated (Mayr & Kliegl, 2000). In this model, switching requires actively retrieving a mental set from long-term memory, bringing it into working memory. This model is considerate of the limited capacity of working memory (Miyake & Shah, 1999) and is consistent with two recent findings: rapid shifting between tasks is independent of the number of potentially to-be-switched-to tasks (Logan, 2004) and that switching to complex tasks takes longer than switching to simple tasks (Yeung & Monsell, 2003). Thus, the literature favors the theory that only in-use mental sets are present in working memory (Mayr & Kliegl, 2000).

Although there is reasonable convergence in the literature regarding *what* is controlled (mental sets), debate persists over *how* the disengagement and engagement processes of a set switch play out. Rubinstein et al. (2001) propose a two-stage model that includes goal-shifting and rule-activation. Goal-shifting takes place before a stimulus is presented, and involves bringing goal-relevant information (e.g., that identifies the switched-to task) into working memory. Rule activation takes place in between stimulus identification and response selection, and involves engaging appropriate and disengaging inappropriate rules for selecting a response for the current task. Kleinsorge, Heuer, & Schmidtke (2002) found support for a model that distinguishes between set-selection operations and implementation operations. The former are endogenously controlled and akin to Rubinstein et al.'s (2001) "goal-shifting." The latter are of two classes, an endogenously controlled one that involves encoding task cues and one that is exogenously controlled and similar to Rubinstein's et al.'s "rule-activation." This model is hierarchical, such that the implementation process automatically follows (or is for the most part a consequence of) the highest-order process, set-selection. Mayr and Kliegl (2003) propose a similar two-stage model that includes a retrieval stage, during which a cue triggers the task rules to be retrieved from long-term memory into working memory, and an application stage, in which the task rules are automatically applied at stimulus onset. What these models have in common is a volitional activation/retrieval stage and an automatic application/implementation stage.

Because sustained activation is characteristic of mental sets (Goschke & Kuhl, 1993), passive decay is likely insufficient to rid working memory of a no longer relevant mental set. Rather, deactivation of the now task-irrelevant set through inhibitory processes, or *backward inhibition*, is required (Arbuthnott & Frank, 2000; Mayr & Keele, 2000). This may be especially true for mental sets that have been guiding behavior successfully for some time, and then, in an instant, are no longer appropriate. A series of experiments by Mayr and Keele (2000) revealed that subjects were slower to switch back to a recently abandoned task (compared to a remotely abandoned task), even when a return to the abandoned task set soon after was fully predictable, and this effect was only observed when shifts needed to be endogenously triggered. In summary, top-down contributions to set-switching appear to include activation of the appropriate set and inhibition of competing sets. This is consistent with Stuss and colleagues' (1995; 2005) hypothesized component processes in set-switching.

Another aspect of the *how* question pertains to set selection. As alluded to earlier, both endogenous and exogenous forces are involved in this process (Monsell, 2003; Ruthruff, Remington, & Johnston, 2001). Endogenous, or top-down input may promote selection of the task-relevant mental set through sustained activation of the current set if task continuation or repetition is the goal, and inhibition of the current set/retrieval of a new set when shifting tasks is required. Exogenous, or bottom-up forces involve the automatic activation of a mental set through overlearned stimulus-response mappings. Since these exogenous forces operate irrespective of the subject's goal for good task performance, they may or may not promote selection of the appropriate mental set. If they do favor selection of the task-relevant set, endogenous control would be redundant. If they do not, endogenous input must be sufficient to overcome exogenous forces; using the terminology of Norman & Shallice's (1986) influential model, the deployment of *supervisory attention* must suppress stimulus-driven activation of *schemata*. The usual final outcome of this interaction between endogenous and exogenous forces is, assuming the subject is cognitively intact, the selection of the task-relevant set – the one that optimizes performance (Mayr, 2003).

Since endogenous input is probably applied continuously and adjusted on-line to minimally meet demands (Monsell, Sumner, & Water, 2003; Yeung & Monsell, 2003), the system must somehow "know" when exogenously-triggered responses are inadequate and endogenous modulation is needed. So, we are forced to assume the existence of a cognitive system that monitors the need for executive control functions and then communicates this information to brain regions that implement them. Such a component is necessary to account for the findings that healthy subjects modify their response biases on-line to optimize performance in response to committing errors (e.g., Laming, 1968) or changing task demands (e.g., Logan & Zbrodoff, 1979). To explain these phenomena, several researchers have proposed a system that monitors response conflicts, which trigger the need for increased executive control (Botvinick, Braver, Barch, Carter, & Cohen, 2001; Botnivick, Braver, Yeung, Ullsperger, Carter, & Cohen, 2004; Miller & Cohen, 2001; Norman & Shallice, 1986). For example, when a prepotent but incorrect response and a correct response are both activated, as in the Stroop task, or when two (or more) equally correct responses are activated, as in stem completion tasks, this conflict-monitoring system signals the need for the recruitment of additional conscious attention resources.

Although this conflict-monitoring system specifies how the level of endogenous control is regulated in response to changing task demands, it does not explicate what type of control is exerted or in other words, how conflict is resolved. To fill this void, Goschke (2000; 2003; see also Gruber & Goschke, 2004) proposed a model that accounts for the quality (i.e., not just the quantity) of executive control, and thus answers Hommel et al.'s (2004) *who* question by defining executive control as emergent property complementary cognitive networks. In Goschke's model, an ever-changing environment demands two central properties of the attention system: persistence and flexibility. The former enables intense concentration, even in the face of distracters (e.g., reading an article in a noisy lobby), whereas the latter refers to our ability to interrupt an ongoing activity to pursue a new one (e.g., dropping the magazine and leaving the building upon hearing the fire alarm ring) when it becomes advantageous. These properties are achieved by a context-sensitive balance between two control processes, one that serves the stable maintenance of a mental set over time and resistance of distraction, and another that promotes openness to potentially relevant information (i.e., background monitoring) and a disposition to quickly reconfigure set if the need arises. These processes are antagonistic, such that ramping up one incurs a cost to the other. Therefore, adaptive executive control requires a balance between persistence and flexibility, and pathological executive control is characterized by a dysregulation of this balance. A similar but less elaborated model put forth by Cohen, Aston-Jones, & Gilzenrat (2004) posits a system that receives input from the conflict-monitoring system and passes on signals to promote either "exploitation" (selective attention to task-relevant features) or "exploration" (permitting the processing of task-irrelevant features), depending on the task demands.

Conceptualization of executive control as a persistence-flexibility dilemma has been documented previously, such as Mesulam's (1999) dictum that an "interplay between concentration and distractibility is one of the most essential prerequisites for advanced mental activity" (p. 238). Moreover, the paradoxical consequences of impaired executive control, perseveration and distractibility, have been known for some time. In fact, in the first detailed case report of a patient who incurred frontal lobe trauma, Harlow

(1868) describes post-injury Phineas Gage as "pertinaciously obstinate, yet capricious and vacillating" (pg. 339). Nevertheless, Goschke appears to be the first to propose a plausible account for how the persistence-flexibility trade-off is dynamically regulated. As such, his model offers great promise in advancing our understanding of executive control, and more generally, purposive behavior. Before this model is widely adopted, however, its assumptions will need to be clearly identified and empirically validated.

The task-switching paradigm has proved successful for evaluating theoretical models of executive control. It enables cognitive scientists to study executive control processes with better experimental rigor and has become the tool of choice (Miyake et al., 2000; Monsell, 2003), espoused over those developed for clinical purposes, such as the Wisconsin Card Sorting Task (WCST; Grant & Berg, 1948; Nelson, 1976). Such clinical measures are highly multifactorial and likely recruit executive control to some extent, but undoubtedly also involve an array of other cognitive abilities (e.g., Ridderinkhof, Span, & van der Molen, 2002). There are several variations of the task-switching paradigm, but all require subjects to rapidly switch back and forth between performing two simple tasks (e.g., classification as odd/even or greater/less than 5) on a set of bivalent stimuli (e.g., single digits). Since both tasks can be performed on the same stimuli, presenting a target stimulus does not uniquely indicate a particular task. The to-be-performed task is specified on a trial-by-trial basis by providing an explicit cue prior to or concurrently with the stimulus onset (*random-cued paradigm*; Meiran, 1996), making the task order entirely predictable (*alternating runs paradigm*; Rogers & Monsell, 1995), or allowing subjects to determine the task order (Arrington & Logan, 2004). Performance is typically measured by response time (RT) and accuracy. The observed effects of manipulating various parameters such as stimulus type, intertrial intervals, etc., on repetition (i.e., A*A*) and switch (i.e., A*B*) trial performance can inform theoretical constructs.

In the context of the task-switching paradigm, the specific assumptions of Goschke's model can be laid out. First, the model posits that preparatory processes are not specific to switch trials. Rather, it predicts that a preparatory state of persistence – perhaps involving sustained activation of the current set and inhibition of all task-irrelevant information – can facilitate performance on repetition trials. Second, the model proposes that switching tasks can be assisted by a general preparatory state of flexibility. This state may be characterized by sensitivity to information irrelevant to the current task and a readiness to quickly shift mental set if the need arises. Third, the model presumes that these preparatory states are antagonistic, such that persistence incurs a cost to flexibility and vice versa. In other words, experimental conditions that lead to improved repetition trial performance should also be associated with decrements in switch trial performance, and vice versa. Fourth, the model presumes that the balance between persistence and flexibility is regulated in response to changing environmental conditions. Thus, covert changes to the task context should induce an adjustment to this balance in order to optimize performance.

Some previously reported research findings have relevance to these assumptions. Consistent with the first prediction, allowing for preparation by lengthening the interstimulus interval (up to one second or so) speeds up RT not only on switch trials, but on repetition trials as well, albeit not to the same degree (Rogers & Monsell, 1995; Meiran, 1996). This modest benefit of preparation time is likely limited by the difficulty level of the tasks used in these studies, and would likely be larger if ceiling effects could be avoided. To further explore the preparatory process involved in task repetition, Ruthruff et al. (2001) employed the alternating runs version of the task-switching paradigm (i.e., AABBAA), but occasionally (on 13.3% of trials) violated the task order to create unexpected repetition (i.e., AABB*B*A) and switch (i.e., AABBA*B*) trials. They used two sets of univalent stimuli, each with four possible responses mapped on to two keys. The response-stimulus interval was 1500 - 1700 ms, allowing for adequate preparation time. Repetition trials were performed faster than switch trials (a repetition main effect) and expected tasks were performed faster than unexpected tasks (a probability main effect). Importantly, these two factors did not interact. That is, foreknowledge of the upcoming task/trial type similarly facilitated repetition and switch trial performance, indicating that time-consuming preparatory processes are involved in both trial types. Another study compared conditions with predictable versus random task-ordering and found that foreknowledge of the upcoming task (possible only in the predictably ordered block) sped up RT similarly on both repetition and switch trials (Sohn & Carlson, 1998). All of these findings are in line with the first assumption of Goschke's model – that preparatory processes are not specific to switch trials.

With regard to the second assumption, helpful preparation for switch trials is suggested by the common finding that switch trials are performed faster at longer interstimulus intervals. In their seminal paper, Rogers and Monsell (1995) showed that switch costs decrease (i.e., the relative RT advantage for repetition trials over switch trials diminished) with increasing response-stimulus intervals, and eventually leveled out at about 600 ms. This benefit from preparation time has been replicated several times in other paradigms (Arrington & Logan, 2004; Kray & Lindenberger, 2000; Meiran, 1996;

but see Koch, 2003), and has been interpreted to reflect active preparation for the upcoming switch trial via time-consuming executive control processes (Rogers & Mosell, 1995) or passive decay of interference from the previous trial (Allport, Styles, & Hsieh, 1994). Because the duration of the preparation period was confounded with remoteness from the previous trial in both Allport et al.'s (1994) and Rogers and Monsell's (1995) studies, the effect of reduced switch costs could not be unequivocally attributed to active preparation or passive decay. To resolve this, Meiran (1996) used the random-cued paradigm, in which these factors can be unconfounded. He found that switch costs were reduced by lengthening the cue-stimulus interval (CSI), thereby unequivocally demonstrating that active preparation prior to task execution was at least partly responsible for the RT reduction. Meiran, Chorev, and Sapir (2000) expanded on this finding by showing that both active reconfiguration and passive decay of the previous task set contributed to the reduction of switching costs with preparation time. Using the random-cued paradigm, they first prolonged the response-cue interval (RCI) and observed a switch cost reduction, consistent with passive decay. They then prolonged the CSI and observed a further reduction in switch costs, consistent with active preparation.

Rather than vary the amount of time to prepare, Sohn and Carlson (2000; Experiment 3) manipulated subjects' knowledge about the forthcoming task. They compared conditions in which the task type on the forthcoming trial (letter, digit, or symbol classification) was entirely predictable vs. random. In both conditions, the upcoming trial type (repetition or switch) was entirely predictable. Note that for repetition trials, the upcoming task type was necessarily predefined (i.e., *AA*) in both conditions. However, for switch trials, it was not predefined in the random condition

(A*B* or A*C*, with equal probability) because the authors used three tasks. Foreknowledge interacted with switch cost, suggesting that knowing the specific identity of the upcoming task facilitated preparation over and above knowing only that a switch trial was forthcoming. In other words, task-specific preparation is more beneficial than generic preparation for switch. Since this study did not include a condition in which subjects had no foreknowledge of both the upcoming trial type and task type, it remains to be determined whether generic preparation for a switch is more beneficial than no preparation, which is exactly what Goschke's model predicts.

Getting closer to this issue, Dreisbach, Haider, and Kluwe (2002) cleverly manipulated subjects' trial-by-trial expectancies by providing probability cues before each trial. This cue indicated, with a probability of .25, .50, .75, or 1.00, which task was to be performed on the forthcoming trial. These authors used two sets of stimuli (numbers and letters), and each was associated with two different tasks (odd/even, greater/less than 7; consonant/vowel, before/after M). The stimuli appeared in a color that specified a particular task. With a RCI of 2,000 ms and a CSI of 1,500 ms, Dreisbach et al. (2002) found that probability had similar effects on repetition and switch trial performance (a null interaction) – decreasing RT with increasing probabilities. Repetitions were performed faster than switches across the range of probabilities. In a second experiment, they replicated these findings using univalent stimuli (four tasks associated with four unique stimulus sets). A subsequent experiment used the identical stimulus set as in the first experiment and probability cues that were either the same as in their first experiment or were only semispecific – indicating an upcoming task-switch without specifying which of the three tasks was to be performed. The instruction indicating which of these tasks to perform came simultaneously with stimulus presentation. This experiment replicated the results of the first one, finding that probability had a similar effect on both repetition and switch trials. However, for the semispecific cues, the authors found an interaction between trial types – the probability cues had an effect on repetition trials (as with the specific cues) but not switch trials. In a final experiment using the same stimuli but a broader range of probability cues (1.0, .75, .50, .25) and only semispecific cues, the authors similarly found linear decreases in repetition trial RT with increasing certainty of an upcoming repetition, but no probability effects for switch trials. Dreisbach et al. (2002) concluded that preparation for a task switch is not possible without knowledge of the specific to-be-switched-to task.

Note that a lack of preparation for upcoming switch trials without foreknowledge of the to-be-switched-to task is incompatible with the second assumption of Goschke's model. Rather, the model posits a more general state of "readiness to switch to different tasks" (Goschke, 2003, p. 70), in which individuals loosen their attentional focus on the current task and monitor their environment for a set-switch signal. Therefore, performance on a switch trial should be faster when it is anticipated versus unexpected, even if the to-be-switched-to task is not revealed in advance. As a real-world example, imagine you are reading the newspaper on a park bench. With previous knowledge that a serial killer is at large in the neighborhood, you would probably focus less on the newspaper and glance at passer-bys with greater scrutiny (i.e., a flexibility for persistence trade-off). Is knowledge of your action upon observation of a suspicious person (e.g., run away, alert the authorities on your cellular phone, pretend to keep reading, etc.) a necessary prerequisite for this state? Introspection suggests not.

If preparation for a task switch without knowledge of the specific forthcoming task is indeed possible, one can only speculate about the processes that underlie it. Recall that the research literature converges on a two-stage model of task-switching that involves activation/retrieval of the new task and inhibition of the no-longer-appropriate task. Clearly, the mental set associated with a new task could not be retrieved before its identity has been revealed (unless subjects luckily guess or have a valid reason to expect a particular task, e.g., unequal probability). It is conceivable, however, that subjects might inhibit the mental set associated with the just-performed task in the case of an expected switch. This would help reduce interference with whichever task is to be performed on the forthcoming trial. However, there is some evidence to suggest that backward inhibition occurs only with activation of the new task set (Dreisbach et al., 2002; Hubner, Dreisbach, Haider, & Kluwe, 2003; Mayr & Keele, 2000), or only with response-related processes (Shuch & Koch, 2003).

Mayr & Keele (2000, Experiment 3) had subjects switch to one of three tasks on every trial in an unpredictable order. A switch was thus guaranteed on each trial, but the identity of to-be-switched-to task was unknown until explicitly cued 700 ms prior to or simultaneously with presentation of the target stimulus. Backward inhibition – slower RT when a task was performed recently versus more remotely – was found only in the pre-cued condition, suggesting that knowledge of trial transition alone was insufficient to bring about inhibition of just-engaged mental set. Hubner et al. (2003) had participants switch between three tasks that were associated with three sets of univalent stimuli. Stimulus presentation was sometimes preceded by task cues (presented 500 ms following a response) that either specified the forthcoming task or merely that a switch was forthcoming. The target stimulus then appeared 1500 ms later flanked by stimuli from either the preceding task or a control task, or not flanked. Interference from flankers was reduced by the specific tasks cues, but not by the cues merely indicating a task switch without specifying the to-be-performed task in advance. Taken together, it appears that knowledge of the specific forthcoming task is required for backward inhibition.

If not activation/retrieval of the new set or backward inhibition, what processes could facilitate unspecific preparation for a task switch? Goschke (2000; 2003; Gruber & Goschke, 2004) states that enhanced sensitivity to potentially relevant novel stimuli may be achieved by lowering the threshold of accessibility to working memory. In the task-switching paradigm, enhanced sensitivity may involve a bias to process one attribute of bivalent stimuli, such as the letter in the stimulus "G4" if the odd/even task was just performed and a task-switch is expected to be forthcoming. Desimone & Duncan (1995) provide a mechanism for how this might be achieved, at least in the visual domain: an "attentional template" biases processing in the visual cortex to selective stimulus properties or to screen out unwanted stimulus features (e.g., shape, color, location). Interestingly, in the Dreisbach et al. (2002) study that found no evidence for a general preparatory state, the experimental design may have disallowed this possibility by using four tasks that were associated with two stimulus sets, letters and numbers. Therefore, with foreknowledge of a task switch, selective attention to one aspect of the stimulus (e.g., numbers) would not be advantageous. As a result, their conclusion that "unspecific preparation for a task shift does not seem possible" (Dreisbach et al., 2002, p. 480) may be premature.

Another plausible means for unspecific task-switch preparation may be to keep all potential mental sets partially activated (Gruber & Goschke, 2004). A theoretical foundation for this hypothesis is found in guided activation theory (Cohen et al., 2004; Miller & Cohen, 2001) and its elaborations (e.g., Gilbert & Shallice, 2002), which propose that partial activation facilitates responding to a particular stimulus attribute by lowering the activation threshold for the mental set associated with that attribute. Thus, potentially relevant mental sets will require less stimulus-driven activation to be loaded into working memory, and so will be more readily selected over competing mental sets. This theory, however, has not been extended to account for partial activation of multiple (competing) sets, a situation that may cause crosstalk and thus be maladaptive (Ruthruff et al., 2001), as mentioned earlier. In conclusion, research to date has not been able to demonstrate clear evidence of a general "flexibility" state (the second assumption of Goschke's model), nor delineate the cognitive processes that characterize it. Nevertheless, several possibilities are tenable, and will be explored in the present study.

A few studies have also examined the trade-off between persistence and flexibility, the third assumption of Goschke's model. Dreisbach & Goschke (2004) looked at the modulating influence of positive affect on these processes, in light of its prior association with increased dopaminergic transmission in the prefrontal cortex. They first trained subjects to respond to stimuli in a given color while ignoring stimuli in another color. In a second phase, subjects then had to respond to either a new color with distracter stimuli appearing in the previously task-relevant color (the perseveration condition) or respond to stimuli in the previously task-irrelevant color with distracter stimuli appearing in a new color (the learned-irrelevance condition). The authors experimentally induced positive affective states by briefly presenting emotionally salient pleasant pictures just prior to the target stimulus on every trial. Compared to conditions in which subjects were shown neutral or negative affect-inducing pictures, these subjects showed reduced switch costs (second phase RT minus first phase RT) in the perseveration condition and increased switch costs in the learned-irrelevance condition. The authors concluded that this pattern indicates increased flexibility at the expense of distractibility. Dreisbach et al. (2005) applied this same paradigm to healthy individuals who differed on neurobiological markers of central dopaminergic function. Two indicators of elevated dopamine levels, high spontaneous blink rates and the presence of the DRD4 exon III 4/7 genotype, were associated with increased flexibility but reduced persistence. Yogev, Hadar, Gutman, and Sirota (2003), apparently unaware of Goschke's model, developed a paradigm similar to the WCST that measured both perseveration and distractibility. Relative to controls, schizophrenics with mostly negative symptoms tended to commit more perseverative errors whereas those with mostly positive symptoms evidenced over-switching, as measured by a ratio between the number of switching responses to the total number of trials. Together, these studies provide evidence that dopamine activity modulates the flexibility-persistence balance. However, of potential importance, so-called flexibility-persistence trade-offs in each of these studies were observed between-subjects or within-subjects but under different experimental conditions, rather than simultaneously within the same subjects. In other words, flexibility- and persistence-related processes were not demonstrated to be truly anatagonistic.

The least progress has been made towards the model's fourth prediction – the persistence-flexibility balance is context-sensitive such that adjustments are made on-line in response to changing task demands. To date, only explicit external cues have been used to manipulate task context (e.g., Dreisbach et al., 2002), easing the demands on exogenously driven processes. In order to adequately test this hypothesis, one would need to demonstrate that subjects engage in preparatory processes to facilitate repetition or switch trial performance, whichever is most adaptive, without being overtly instructed to do so on a trial by trial basis. This is another aim of the present study.

In summary, executive control research to date has primarily focused on the discrete process of a task shift – the "reconfiguration" involved in disengaging from one task and switching to another. Clearly, adaptive functioning in the real world requires not only the ability to shift our attention when instructed, but also to identify when it is appropriate to shift versus maintain focus and implement these adjustments as we go about novel goal-directed behavior. The next step in executive control research, then, must be to characterize the brain systems that regulate the balance between set-switching and set-maintenance, and theoretically integrate this system with existing models of executive functioning (e.g., Norman & Shallice, 1986; Miller & Cohen, 2001). Goschke has taken an important step in this direction by proposing a theoretical account for how the persistence-flexibility balance is dynamically regulated. This model is broadly consistent with most available cognitive and neurophysiological research to date. However, several of its fundamental assumptions have been subjected to little direct empirical inquiry, and the model therefore remains "speculative" (Goschke, 2000, p. 351). The main goal of the present study is to empirically evaluate Goschke's theory by testing its assumptions in a task-switching paradigm. The present results may also force expansion of computational models of executive control (Cohen et al., 2004; Gilbert &

Shallice, 2002) to accommodate more than two mental sets, or "task demand units," and clarify some previous equivocal empirical findings (e.g., Dreisbach et al., 2002). Other potential benefits of this research include improving the clinical assessment of executive control and helping to characterize attention deficits in various clinical populations. Individuals with Attention-Deficit/Hyperactivity Disorder, for example, may be capable of persistence and flexibility but unable to infer the optimal balance and so maintain set when it is counterproductive (i.e., perseverate) and switch set when focus is needed (i.e., show distractibility).

To evaluate the four main assumptions of Goschke's model outlined above, I used the random-cued version of the task-switching paradigm[1] and manipulated participants' expectancies about the forthcoming trial (what to prepare for) to encourage persistence- or flexibility-related preparatory processes. Since explicit cues indicating the probability of an upcoming task (as in Dreisbach et al., 2002) are largely nonexistent outside the laboratory and ease the demands on executive control, I wished to develop a more ecologically valid paradigm in which the task context could be altered without explicitly instructing subjects to modify their preparatory strategy on a trial by trial basis. One way to do this is to manipulate the frequency and thus subjective probability of switch/repetition trials. When repetition trials are relatively common, set-maintenance/persistence related processes should be active during the RCI. When switch trials are common, deploying set-shifting/flexibility related processes during each RCI would maximize efficiency on most trials.

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[1] Even though the random-cued version of the task-switching paradigm is not generally associated with top-down control (Logan & Bundensen, 2003), manipulating participants' expectancies regarding the forthcoming trial and lengthening the RCI will allow for the activation of top-down processes prior to the task cue (i.e., during the RCI).

General Method

Participants

All participants were undergraduate students at the University of Windsor and were awarded bonus course credit as compensation. They spoke English fluently and did not meet any of the following exclusion criteria (by self-report): (1) history of head trauma or other neurological illness, (2) formal diagnosis of learning disability or Attention-Deficit/Hyperactivity Disorder, (3) current mood, anxiety, or psychotic disorder, (4) color blindness.

Materials

The stimulus set resembled the WCST (Grant & Berg, 1948) cards. They were comprised of boxes containing geometric figures that differed on three dimensions: color (red, yellow, blue, and green), shape (square, circle, triangle, and plus sign), and number (one to four). Four reference stimuli were made up of orthogonal combinations of the three dimensions: one green square, two blue plus signs, three red circles, and four yellow triangles. These are shown in Figure 1. The experimental task was to match target stimuli to the correct reference stimulus based on a specified dimension (e.g., color). The target stimuli were 24 unique combinations of the three dimensions that shared attributes with three out of the four reference stimuli[2]. Thus, each target stimulus was the same color as one of the reference stimuli, the same shape as another, the same number as a third, and bore no resemblance to the last one (e.g., see Figure 1). Since the task-irrelevant features (e.g., color and number if the shape task is cued) are always associated with different responses, they can be considered "incompatible noise" (Gratton

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[2] All possible combinations of four levels of the three dimensions (color, shape, and number) yield 64 distinct stimuli. Four of these are identical to the reference stimuli, 36 overlap with two of the reference stimuli, and 24 (those retained for the present study) overlap with three of the reference stimuli.

et al., 1992).

The four reference stimuli, presented just below each target stimulus in a 400 X 560 pixel display with a white background, corresponded to four keyboard keys (C, V, B, and N). This (topographically congruent) mapping is also illustrated in Figure 1. To indicate a match between a target and the "four yellow triangles" reference stimulus, for example, participants would press the "N" key. Stimulus presentation and data collection were achieved through SuperLab software (Cedrus Corporation), run on an IBM Pentium II desktop computer.

Procedure

The experiment began with written and oral instructions on how to perform each of the tasks and the response-keyboard mappings. Participants then performed a practice pure-block containing 20 trials for each of the three tasks (color, shape, number) and then a practice mixed-block of 60 trials in which task order was random (i.e., 120 practice trials in total). All of these blocks had a CSI of 100 ms and a RCI of 300 ms. After each response, feedback about its accuracy was provided. If correct, the word "*CORRECT*" was displayed for 200 ms. If incorrect, the word "*INCORRECT*" was similarly displayed. Note that this 200 ms was part of the 300 ms RCI.

For the test phase, participants performed several different blocks in a counterbalanced order, each 160 trials in length, with equal representation of the three task types (i.e., approximately 53 trials of each of the color, shape, and number task). The CSI was held constant at 100 ms, as to not allow for post-cue preparation (see Meiran, 1996). That is, any preparatory processes will have to be carried out before the presentation of the cue indicating which task is forthcoming (and therefore should not be task-specific in the case of switch trials). All participants completed a block with 75% switches (and 25% repetitions; referred to as the "high-switch block" hereafter) and one with 25% switches (and 75% repetitions; "high-repetition block"). Both had a RCI of 1000 ms, which has been shown to be adequate for preparation in the task-switching paradigm (Rogers & Monsell, 1995). One second has also been shown to be optimal in prepared RT paradigms (e.g., Stuss et al., 2005). Participants were encouraged to briefly rest in between the blocks.

Trials proceeded as follows. After the RCI, a cue appeared in the middle of the screen to indicate the forthcoming task (*COLOR, SHAPE,* or *NUMBER*). 100 ms later, the stimulus appeared just above the cue and both remained on the screen until a response was made. The four reference stimuli were always presented just below the cue, in the same relative location as the keyboard responses. When a response was pressed, the cue, target stimulus, and reference stimuli all immediately disappeared, signaling the onset of the next RCI. A fixation cross appeared on the screen at the location of the task name for the duration of the RCI. No feedback was provided during this phase of the experiment. This sequence of events is depicted in Figure 2.

Following completion of the experimental blocks, participants were asked several questions as part a manipulation check (see Appendix A) and then debriefed.

Data Analysis

The dependent variables were RT and error rate, measuring speed and accuracy, respectively. Trials were classified as "repetition" trials if the immediately preceding trial (N - 1) involved the same task and as "switch" trials if the N - 1 task was different. Average RT and error rates were computed separately for these two trial types, for each participant, at each combination of levels of the independent variables. Because a preliminary exploration of the data revealed that RTs were positively skewed, within and across blocks, the median was thought to be a more appropriate measure of central tendency. This made the standard practice of deleting outlier trials with an arbitrary cut-off (e.g., with RT > 3000 ms) superfluous. The percentage of errors was also moderately positively skewed and so was transformed via a square root function.

When discussed below, the assumptions for all parametric statistical tests were met except where explicitly stated otherwise. The methodological design ensured equal sample sizes and independence of observations. Departures from univariate and multivariate normality were corrected with data transformations as necessary. In cases where the assumption of sphericity was not met (Mauchly's Test of Sphericity was significant at $p < .05$), the degrees of freedom were adjusted to a more conservative level based on the Greenhouse-Geisser estimate of epsilon. For the analyses that included a between-subjects factor (block order), any heterogeneity of covariance matrices was modest at worst, and should not be problematic given the equal group sizes.

Alpha was set at .05 for omnibus tests. Bonferroni-corrected t-tests were used for pairwise comparisons because they are reasonably powerful when only a small number of contrasts are performed and yet they keep the familywise alpha level at or below .05, even with severe violations of the sphericity assumption (Stevens, 2002). For planned comparisons, alpha was corrected for the number of pairwise tests performed, whereas for post-hoc tests, alpha was corrected for all possible pairwise contrasts.

Also of note, Cohen's *d* was always computed using the pooled standard deviation in the denominator, and the 95% confidence intervals surrounding each mean

RT value in Figures 5 and 6 were computed using Loftus and Mason's (1994) formula for multifactor within-subjects designs, separately for repetition and switch trials.

In each experiment, an outlier analysis was first conducted to identify any participants who may not be representative of the sample, possibly due to inadequate motivation, somnolence, an unreported neurocognitive disorder, or some other factor. Next, a set of exclusion criteria was applied to the trials. The first ten trials of each block were excluded because they were considered a "warm-up" period in which subjects would presumably establish expectancies based on their perceptions of the relative frequency of task repetitions and task switches that would guide the response strategies for the remaining 150 trials of the block. This accounted for a loss of 6.3% of the data. Trials that were errors or immediately followed errors were not analyzed for speed (overall error rates were 6.3%, 7.8%, and 6.9% for Experiments 1, 2, and 3, respectively). This was due to the fact that the task performed on error trials could not be known, and therefore their classification as repetition or switch trials could not be determined; since this classification is based on concordance (or discordance) between the present trial and the N - 1 trial, trials immediately following errors could also not be classified.

Switch trials will be referred to as probable switches in the high-switch blocks and as improbable switches in the high-repetition blocks because the global probability of a switch trial is high and low in these blocks, respectively. Correspondingly, repetition trials will be referred to as probable repetitions in the high-repetition blocks and as improbable repetitions in the high-switch blocks.

Experiment 1

To ensure the involvement of executive control in task preparation, the paradigm must make it so that preparatory processes have to be endogenously triggered rather than explicitly cued. As mentioned above, the task-context could covertly change to promote shifts towards either end of the persistence-flexibility spectrum with an unwarned blockwise manipulation of the repetition:switch trial ratio. Encoding frequency information is automatic (Hasher & Zacks, 1979; 1984)[3]. It is resilient to normal aging (Sanders, Wise, Liddle, & Murphy, 1990) and even dementia of the Alzheimer's type (Wiggs, Martin, & Sunderland, 1997). Previous studies have shown that participants can make use of frequency information to optimize their performance through strategic processing (e.g., Gratton, Coles, & Donchin, 1992; Logan & Zbrodoff, 1979). Therefore, a high frequency of repetition trials (relative to switch trials) should promote persistence-related processes during each RCI, whereas a high frequency of switch trials (relative to repetition trials) should promote flexibility-related processes during each RCI.

A potential problem with this paradigm is the possible confound between the frequency of each trial type and practice effects (Dreisbach et al., 2002). By varying the frequency of switch trials within a block, the amount of practice with switch and repetition trials was also varied. Though participants will have performed the same number of trials of each type by the end of the experiment, they will be differentially practiced with repetition and switch trials at most points during the experiment. No confound is anticipated based on previous research examining practice effects by comparing repeated trials within a run or by comparing blocks over the course of the experiment. Specifically, Rogers & Monsell (1995) found no evidence for micropractice effects within a run of repetition trials. Mean RT for the first, second, and third repetition in a run did not differ. Additionally, Meiran (1996) demonstrated that extensive practice

[3] But can be improved by conscious processing (Sanders, Gonzalez, Murphy, Liddle, & Vitina, 1987).

produces linear decreases in shift costs when no preparation time (defined as the CSI) is provided, but no practice effects were observed when the CSI was long. Therefore, it seems that cue-stimulus associations become strengthened with long-term practice, but preparatory processes do not get more efficient (Meiran, 1996). Another study found decreasing RTs with increasing practice but a null interaction between practice and trial type (switch versus repetition; Dreisbach et al., 2002). In spite of this evidence, to be safe, any effect of long-term practice in the present study was eliminated by counterbalancing (see Methods section). Also, the design of the present study allows for an evaluation of the magnitude of practice effects, since predictions based on practice effects run counter to some of the hypotheses[4].

The blocks with disproportions of switch and repetition trials were compared to a control block that has an equal ratio of switch and repetition trials. Setting the probability of a switch/repetition to 50% should remove the predictability of task order and thus deny subjects foreknowledge of what to prepare for (a repetition or switch). With no foreknowledge of trial order, participants could adopt three different strategies during the RCI (Sohn & Carlson, 1998). They could (1) maintain set on every trial and be well prepared for repetitions but very slow for switches, (2) guess at the next task and prepare for a switch to that task, leaving them in a good position if that task happens to come, but in poor shape otherwise, or (3) not prepare and simply wait for the cue. Participants can be expected to adopt the latter approach based on previous research (e.g., Sohn & Carlson, 1998) and also because it is most effective overall (Gratton et al., 1992).

[4] For example, consider the administration of one block with frequent switches followed by another with infrequent switches. Having just completed the frequent-switching block, subjects would be disproportionately practiced with switch trials. Consequently, switch costs in the subsequent (low frequency) block should be small. However, switch costs would be predicted to be large in the second block on the basis of Goschke's theory.

Contrasts between this equal-proportioned block and the high-repetition/switch blocks should elucidate the distinctions between *preparing to* repeat/switch, *not preparing to* repeat/switch, and *preparing to not* repeat/switch.

Method

Participants

Twenty undergraduate students at the University of Windsor voluntarily participated in this study. They ranged in age from 18 to 27 years (median = 19). The majority (84%) were female.

Procedure

After completing the practice phase (described above), participants worked through the high-switch and high-repetition blocks in a counterbalanced order, before (n = 10) or after (n = 10) the equal-proportioned block. RCI was always 1000 ms. This made for a 3 (block) X 2 (trial type) design. Participants were not informed of the blockwise manipulation. Rather, each block was introduced in the same generic manner, e.g., "here is the next block." Therefore, the block-to-block transitions should have been viewed as mere breaks in a long experiment rather than the end of one set of experimental conditions and the beginning of different conditions.

Results

Data screening

Boxplots of global (across all blocks) mean RTs identified one subject as a clear outlier. This participant fell 5.94 standard deviations above the sample mean. Interestingly, he reported being only "6 out of 10" concerned with answering as fast as he could on the manipulation check questionnaire, which was markedly different from the rest of the sample ($M = 8.1$, $SD = 1.1$). This suggests insufficient motivation or an inordinately strong accuracy-for-speed tradeoff. Regardless, this subject was excluded from the analyses reported below.

Preliminary Analyses

Practice blocks. The practice blocks were first subjected to analysis. A one-way within-subjects ANOVA with task-type as the independent variable produced a significant main effect, $F(1.36, 24.41) = 51.94$, $p < .00001$, $\eta_p^2 = .743$. Bonferroni-corrected paired t-tests revealed significant differences between the shape block and the other two blocks (both $p < .00001$), but no difference between the color and number blocks ($p = .994$). Errors were very infrequent and extremely positively skewed (most participants made no errors). Since all participants completed these three practice blocks in the same order, blockwise comparisons may be complicated by practice effects, and more likely, proactive/retroactive interference. Therefore, the enticing conclusion that the shape task was more difficult than the other two tasks must be withheld for now.

The 60-trial mixed practice block was analyzed next. According to a paired t-test, repetition trials were performed faster than switch trials, $t(18) = 2.79$, $p = .012$, Cohen's $d = .414$. Participants also made more errors on switch trials compared with repetition trials, $t(18) = 4.88$, $p = .00012$, Cohen's $d = 1.346$. In other words, both the RT and error rate data supported significant switch cost effects. A one-way within-subjects ANOVA with post-hoc tests revealed a main effect for task type [$F(2, 36) = 18.19$, $p < .00001$, $\eta_p^2 = .503$]. It also showed that the shape task was performed slower than the color and number tasks (both $p < .0001$), which did not differ from each other ($p = .499$). This finding further lends credence to the hypothesis that the shape task was harder than the other two tasks. Visual inspection of box-plots of RT by trial number for this block (see Figure 3) did not reveal a linearly decreasing trend, suggesting trivial within-block practice effects.

Although the same practice blocks were administered in subsequent experiments, there is no reason to believe that the results would be any different. The above analyses were therefore not repeated for each experiment.

Replication of basic task-switching phenomena. To ensure that there was nothing peculiar about the stimulus set, stimulus-response mappings, matching tasks, or other idiosyncrasies of the method employed in this study, I first examined whether some common findings in the literature would replicate. In virtually all previous studies employing the random-cued version of the task-switching paradigm, the proportion of repetition and switch trials is equal. It therefore seems appropriate to attempt to replicate major findings from these studies by analyzing only the equal-proportioned block in the present study.

The most robust finding in these studies is a large switch cost – switch trials are performed slower than repetition trials (e.g., Meiran, 1996), even when the RCI exceeds one second (Meiran et al., 2000). This hypothesis was tested with a dependent samples t-test, which revealed a significant switch cost of about 100 ms, $t(18) = 4.01$, $p = .001$, Cohen's $d = .775$. The same contrast with the square-root transformed error rates was not quite significant, $t(18) = 1.64$, $p = .118$, Cohen's $d = .369$.

The correct response on a repetition trial may be the same or different from the correct response on the preceding trial. That is, participants may be required to repeat tasks and responses or repeat tasks but press a different response. The same is true for switch trials. To examine the effect of repeating versus switching responses and its potential interaction with repeating versus switching tasks, these variables were entered into a response type by trial type within-subjects ANOVA (for their descriptive statistics, see Table 1). There was a large main effect for trial type [$F(1, 18) = 27.14$, $p < .00006$, $\eta_p^2 = .601$; repetitions < switches], no main effect for response type [$F(1, 18) = .70$, $p = .413$, $\eta_p^2 = .038$; repetitions = switches], and a large interaction effect [$F(1, 18) = 21.16$, $p = .0002$, $\eta_p^2 = .540$]. The latter indicated that response repetitions were facilatory for task repetitions but inhibitory for task switches. For the same ANOVA with error data, there was only a small main effect for response repetition [$F(1, 18) = 6.25$, $p = .022$, $\eta_p^2 = .258$], indicating that errors were higher for response repetitions than response switches. Roger and Monsell (1995) obtained these same RT findings, and offered a few tentative explanations for why they appear. Responding to a stimulus may increase the association between the relevant stimulus attribute category and response while decreasing the association between that response and other stimulus attributes. Alternatively, a mechanism may exist that is designed to prevent perseverative responding. This system might check the planned response against the just-executed response, and if a match is detected, a time-consuming rechecking of the decision would be triggered; repetition priming would more than overcome these inhibitory effects on repetition trials.

An effect of task recency is another seminal finding (Ruthruff et al., 2001). Ruthruff and colleagues realized that switch trials differ with respect to how recently the switched-to task was performed, varying from a minimum of 2 trials ago to 10 or more in many task-switching experiments. They deconstructed the binary repetition-switch trial distinction, instead classifying trials based on the number of preceding trials since the task was last performed (i.e., task recency = 1 for repetitions trials and task recency > 1 for switch trials). The authors found a significant effect for task recency – RT increased linearly with the number of trials since the task was last performed. However, they used univalent stimuli (non-overlapping stimulus sets associated with the different tasks), and there is good reason to speculate that this finding would not replicate with multivalent stimuli (as in the present study). By their own admission, a consequence of using univalent stimuli was that "participants presumably never had to inhibit processing of the inappropriate task" (pg. 1415). With multi-affordance stimuli, backward inhibition is needed to reduce proactive interference (e.g., Mayr & Keele, 2000). The effects of backward inhibition may offset or even outweigh any beneficial effect of task repetition. Consistent with this hypothesis, Figure 4a seems to shows a switch cost (difference between 1 and 2), but no strong trend of linearly increasing RTs from 2 to 5 (within switch trials). There is also no such trend in the error data (see Figure 4b).

As highlighted in the analysis of the practice blocks, the three tasks do not appear to have been equal in difficulty. Moreover, unequal tasks may show asymmetrical switching costs (Allport et al., 1994). A task type by trial type (3 x 2) within-subjects ANOVA revealed large main effects for task type [$F(2, 18) = 58.78$, $p < .00001$, $\eta_p^2 = .874$] and trial type [$F(1, 18) = 18.28$, $p = .00046$, $\eta_p^2 = .504$], as well as an interaction effect, $F(2, 17) = 5.40$, $p = .015$, $\eta_p^2 = .389$. Follow-up post-hoc tests revealed that shape task was performed slower than the color [$t(18) = 11.11$, $p < .00001$, Cohen's $d = 1.593$] and number [$t(18) = 8.67$, $p < .00001$, Cohen's $d = 1.442$] tasks, and that the latter two did not differ, $t(18) = .43$, $p = .68$, Cohen's $d = .061$. Post-hoc exploration of the interaction effect revealed that the only significant difference was between the switch cost for the number and color tasks, $t(18) = 3.45$, $p = .003$, Cohen's $d = .783$. Descriptive statistics are presented in Table 2. Errors were too infrequent and extremely positively skewed, so only group means are reported. Switch costs appear smallest for the color task, largest for the number task, and intermediate for the shape task. Allport et al. (1994) suggested that more overlearned task sets (e.g., reading words) require greater backward inhibition than novel task sets (e.g., naming the color of the ink words are printed in), and are therefore harder to switch back to, since this extra inhibition must be overcome. It is not intuitively obvious why classifying objects based on their quantity, by this account, is a more overlearned task than classifying them based on their shape, and so on. This is especially odd given that the difficulty gradient (in terms of overall RT) for these three tasks followed a different pattern. Importantly, the task distribution was balanced in all experimental blocks, so even widely discrepant switch costs would not systematically influence the below results.

Main Analyses

RT data. Descriptive statistics are provided in Table 3. A 3 (block) X 2 (trial type) within-subjects ANOVA revealed a main effect for trial type [$F(1, 18) = 41.06$, $p < .00001$, $\eta_p^2 = .695$], indicating that repetitions were performed faster than switches. The main effect for block was not significant [$F(2, 17) = .31$, $p = .741$, $\eta_p^2 = .035$], suggesting that the overall mean RT for high-switch, equal-proportioned, and high-repetition blocks did not differ. There was also a robust interaction effect [$F(2, 17) = 16.90$, $p = .00009$, $\eta_p^2 = .665$], plotted in Figure 5. This interaction reflected decreasing switch costs with increasing probability of a switch trial – they were 182, 104, and 74 ms in the high-repetition, equal-proportioned, and high-switch blocks, respectively. Planned comparisons were used to examine the probability effects more closely. Only the mean repetition RTs for the high-switch and high-repetition blocks significantly differed, $t(18) = 3.17$, $p = .005$, Cohen's $d = .545$. That is, probable repetitions were performed faster than improbable repetitions. All other comparisons were in the predicted numerical direction, but non-significant at a Bonferonni-corrected alpha of .017.

In most paradigms (including the present one), repetition and switch trials are defined by the immediately preceding trial, ignoring events before it. A given trial is simply classified as a repetition trial if the previous trial involved the same task and is classified as a switch trial if the previous trial involved a different task. This method equates repetition trials that are preceded by one, two, three, or more trials involving the same task, which may be inappropriate if there are systematic RTs trends within runs of repetitions or switches (Meiran et al., 2000). In the present data set, however, there was a small effect size between the mean RT for the first trial of a run of repetitions and later trials in a run (averaged), Cohen's $d = 0.237$. The effect size for the same comparison, but with switch trials, was only 0.002. Spearman's rho correlations between RT and position in a run (as a continuous variable) were also very low, -.126 for repetition and -.015 for switch trials. Since RT did not appear to systematically differ by position in a run for either repetition or switch trials, omitting trials after the first position in the run should have little effect on the results. When they were omitted, the same pattern of results was obtained. Specifically, a within-subjects ANOVA revealed a main effect for trial type [$F(1, 18) = 31.27$, $p = .00003$, $\eta_p^2 = .635$], a non-significant main effect for block [$F(2, 17) = .42$, $p = .664$, $\eta_p^2 = .047$], and a strong interaction effect, $F(2, 17) =$

6.21, $p = .009$, $\eta_p^2 = .422$. For these reasons, all subsequent analyses will include trials in any position of a run, i.e. adopt the standard definition of repetition and switch trials. Another reason for this decision is that limiting measurement of performance on repetition trials to the first trial of a run will underestimate the switch cost whenever task order is not fixed (Monsell, Sumner, & Waters, 2003), as in the present paradigm.

Because expectancy may well carry over from one block to the next, experimental block order should be analyzed as another independent variable. Participants who completed the high-switch before the high-repetition block were compared to those who completed these two blocks in the opposite order. In a block X trial type X block order split-plot ANOVA (the equal-proportioned block was not included in this analysis to enhance interpretability), the only significant main effect was for trial type, $F(1, 17) = 45.72$, $p < .00001$, $\eta_p^2 = .729$. In contrast, RT did not differ by block [$F(1, 17) = .55$, $p = .466$, $\eta_p^2 = .032$] or block order [$F(1, 17) = .62$, $p = .44$, $\eta_p^2 = .035$]. The block by trial type interaction was significant, $F(1, 17) = 32.91$, $p < .00001$, $\eta_p^2 = .659$, but the other two-way interactions were both $p > .05$. The three-way interaction was also non-significant [$F(1, 17) = 1.23$, $p = .283$, $\eta_p^2 = .067$], suggesting that the probability effects were similar for both block orders. However, post-hoc tests revealed isolated probability effects for switch trials in the subsample that completed the high-repetition block first [$t(8) = 4.45$, $p = .002$, Cohen's $d = 1.00$], and isolated probability effects for repetition trials in the subsample that completed the high-switch block first [$t(9) = 3.40$, $p = .008$, Cohen's $d = .681$].

One possible line of explanation to account for this finding is that subjects who performed the high-switch block first may have reached ceiling levels on switch trials and so benefited no further in the second block, whereas their performance on repetition trials (of which they performed few in the first block) improved in the subsequent high-repetition condition. Likewise, subjects who performed the high-repetition block first may have achieved ceiling RT on repetition trials but not switch trials, and then showed improvement only on switch trials in the second block. Of course, this would only be plausible if practice effects were minimal for the less frequent trial type within the first block and then substantial within the second block, when that trial type was more frequent. The data do not support this hypothesis. The high-switch and high-repetition blocks were divided into quarters. Because there were not enough valid trials (only about 5 to 7, on average) to compute reliable means for a within-subject analysis, only the group level data was analyzed. Means for the groups who performed the high-repetition block first and the high-switch block first were computed for the second (46th to 80th trial) and fourth (116th to 150th trial) quarters of each block, separately for repetitions and switches. A practice effect would be indicated by a significant RT decrease from the second to the forth quarter. Within-block RT differences were small and inconsistent (some in the numerical direction of practice effects and others in the direction of fatigue effects). Alternative explanations are explored in Experiment 3.

Error rates. The block by trial type within-subjects ANOVA revealed a main effect for trial type [$F(1, 18) = 11.84$, $p = .003$, $\eta_p^2 = .397$] but not block [$F(2, 17) = 1.66$, $p = .22$, $\eta_p^2 = .164$]. The interaction effect was also significant, $F(2, 17) = 4.54$, $p = .026$, $\eta_p^2 = .348$. None of the pairwise contrasts reached significance. Of note, the numerical trends for the (transformed) error rates were in the opposite direction as predicted (and the RT findings) – switch costs were largest in the high-switch block (1.2% error rate difference), and smaller in the equal-proportioned (.4%) and high-repetition (.3%) blocks, suggesting that a speed-accuracy tradeoff may contribute to (but not fully explain) the RT results. As with the RT data, excluding all trials that were not in the first position of a run did not significantly alter the results. A block X trial type X block order split-plot ANOVA revealed that block order did not influence the probability effects.

Errors on switch trials can be classified as perseverative or non-perseverative (i.e., random) in a similar manner to the WCST. An incorrect response on a switch trial is classified as perseverative if it would have been correct for the task cued on the previous trial[5]. Such an error may occur as the result of maintaining set during the RCI and getting "caught" erroneously applying this set to the forthcoming target stimulus, i.e., performing the no-longer-appropriate task. Since there are three possible incorrect response alternatives on a given switch trial, the chance probability of an error being of the perseverative type is about 33%. The percentage of all switch trial errors that were perseverative errors were calculated for each block: 48.1% in the high-switch block, 68.4% in the equal-proportioned block, and 69.5% in the high-repetition block. According to chi-square tests, the high-switch block significantly differed from the equal-proportioned block [χ^2 (1) = 10.05, p = .0015], but the equal-proportioned block did not differ from the high-repetition block [χ^2 (1) = .02, p = .882]. Thus, perseverative errors were far more common in the blocks where repetitions occurred with at least equal frequency to switches. Of note, all of these values fell outside of the 95% confidence intervals of the binomial distributions based on a .33 probability of a perseveration and

[5] It should be noted that Meiran & Daichman (2005) appropriately criticized this method of classifying errors. They argued that a so-called perseverative error (as classified above) may indeed be an instance of correctly performing the incorrect task, however, it may also indicate an incorrect response to the correct task – there is no way to tell the difference.

where the number of events was the total number of errors committed (which varied across the blocks). In fact, the binomial probability of obtaining the observed number of perseverative errors was less .001 for all three blocks.

Manipulation check. When asked if they noticed any differences between the different parts (blocks) of the experiment, only one quarter (26%) reported a difference in the proportion of repetition and switch trials.

Summary of Main Findings

In the present experiment, the probability of a switch/repetition was covertly manipulated within-subjects. The pattern of RTs indicated an interaction between probability of a switch/repetition and trial type (switch vs. repetition), with large and statistically robust switch costs (182 ms) in the high-repetition block and negligible switch costs (74 ms) in the high-switch block. Detailed exploration of this interaction revealed probability effects (probable RT < improbable RT) for repetitions, but not switches. RTs for unpredictable repetition trials fell intermediate to probable and improbable repetition trials, but were not statistically distinguishable from them. Switch trials exhibited the same pattern. When block order was added as a between-subjects variable, it modified the pattern (but not the magnitude) of the probability of a switch/repetition by trial type interaction. Specifically, isolated probability effects for repetition trials were found at one block order and isolated probability effects for switch trials were found at the other block order. Finally, perseverative errors were found to be less likely in the high-switch block compared to the other two blocks.

Experiment 2

As Yeung & Monsell (2003) point out, a comparison between short and long interstimulus intervals is necessary to be sure participants made use of the preparation time. It can also rule out exogenous factors as (at least partially) explaining the pattern of findings in Experiment 1. If the probability effects reported above were actually endogenously driven, as hypothesized, they should disappear when preparation time is not provided, since under these conditions, participants will have some foreknowledge of the upcoming trial type but will not be able to make use of this knowledge, since active preparation is time-consuming. In statistical terms, the interaction between trial type and block (probability of a switch/repetition) demonstrated above (when RCI = 1000 ms) should not be replicated when the RCI is only 100 ms. As well, if top-down set-maintenance processes were responsible for the elevated rate of perseverative errors in the high-repetition block in Experiment 1, this type of switch error should be no more frequent in the high-repetition block than the high-switch block when the RCI is short.

Method

Participants

Twenty undergraduate students at the University of Windsor voluntarily participated in this study. They ranged in age from 18 to 23 years (median = 19). The majority (58%) were female.

Materials

Identical to Experiment 1.

Procedure

After completing the practice phase (described above), participants worked through high-switch and high-repetition blocks at an RCI of 1000 ms in a counterbalanced order, before ($n = 10$) or after ($n = 10$) high-switch and high-repetition blocks at an RCI of 100 ms, also in a counterbalanced order. As in Experiment 1, they were not informed of these blockwise manipulations.

Results

Data Screening

Boxplots of global (across all blocks) mean RTs did not identify any participants as outliers. All twenty participants were therefore retained for the analyses below.

RT Data

Descriptive statistics are provided in Table 4. A three-way completely within-subjects ANOVA was conducted, with trial type, percent switch, and RCI as independent variables. This procedure yielded significant main effects for trial type [switch > repetition; $F(1, 19) = 67.11, p < .00001, \eta_p^2 = .779$], block [high-switch > high-repetition; $F(1, 19) = 11.41, p = .003, \eta_p^2 = .375$], and RCI [short > long; $F(1, 19) = 17.33, p = .00053, \eta_p^2 = .477$]. None of the interactions were significant, except for trial type by block, $F(1, 19) = 9.64, p = .006, \eta_p^2 = .337$. To more closely examine the trial type by block interaction, separate 2 X 2 ANOVAs were run for each RCI. Importantly, there was a significant interaction at the long RCI [$F(1, 19) = 12.66, p = .002, \eta_p^2 = .400$] but not at the short RCI [$F(1, 19) = 2.27, p = .148, \eta_p^2 = .107$]. Pairwise contrasts indicated that the former interaction was driven by probability effects for repetition trials [$t(19) = 4.12, p = .001$, Cohen's $d = .748$], i.e., probable repetitions for performed faster than improbable repetitions. Probability effects for switch trials were non-significant [$t(19) = .04, p = .97$, Cohen's $d = .008$].

This null probability effect for switch trials reflects an averaging of modest probability effects in the group that completed the high-switch block second [$t(9) = 1.61$, $p = .142$, Cohen's $d = .410$] and a *reverse* effect for those who completed the high-switch block first [$t(9) = 2.32$, $p = .046$, Cohen's $d = .507$]. Also of note, probability effects for repetitions were considerably stronger in the group that completed the high-repetition block second [$t(9) = 4.71$, $p = .001$, Cohen's $d = 1.20$] compared to those who completed it first [$t(9) = 1.69$, $p = .126$, Cohen's $d = .387$]. Thus, the data for the long RCI conditions followed the same pattern as in Experiment 1, with block order modifying the nature of the interaction between block (probability of a switch/repetition) and trial type.

Error Rates

The three-way ANOVA with transformed error data revealed main effects for RCI [short > long; $F(1, 19) = 6.96$, $p = .007$, $\eta_p^2 = .326$], trial type [switch > repetition; $F(1, 19) = 5.06$, $p = .031$, $\eta_p^2 = .223$], but not block [$F(1, 19) = .32$, $p = .488$, $\eta_p^2 = .025$]. The trial type by block [$F(1, 19) = 5.27$, $p = .033$, $\eta_p^2 = .217$] and trial type by block by RCI [$F(1, 19) = 4.26$, $p = .008$, $\eta_p^2 = .318$] interactions were also significant.

At the long RCI, perseverative errors were far more common in the high-repetition block (77.4%) than in the high-switch block (52.2%) [χ^2 (1) = 12.48, $p = .0004$], as in Experiment 1. However, as predicted, this pattern did not hold in the short RCI conditions [χ^2 (1) = .12, $p = .73$]; the percentage of switch errors that were of the perseverative type was similar in the high-repetition (58.6%) and high-switch (56.3%) blocks.

Manipulation Check

When asked if they noticed any differences between the four different parts (blocks) of the experiment, 65% spontaneously reported a varying RCI and 40% reported noticing a difference in the proportion of repetition and switch trials.

Summary of Main Findings

This experiment replicated the probability of a switch/repetition by trial type interaction when the RCI was long (as in Experiment 1), but showed that this interaction could be sharply attenuated by reducing the RCI. The difference between perseverative errors in the high-repetition and high-switch blocks was also replicated at the long RCI but eliminated at the short RCI.

Experiment 3

This experiment was essentially a replication of Experiment 1, except that participants were explicitly informed of the characteristics of each block (i.e., the probability of a repetition/switch) and instructed to make use of this information as best they could. This follow-up study was conducted for several reasons. First and foremost, overtly manipulating expectancies may clarify the above results to the extent that it produces larger effect sizes and more clear-cut patterns. Since the equal-proportioned switch block did not significantly differ from the other two in Experiment 1, it is unclear whether the observed probability effects were due to RT-costs (i.e., slowing on improbable repetitions/switches) or -benefits (e.g., facilitation on probable repetitions/switches) relative to a baseline – repetitions and switches in the equal-proportioned block. That is, the distinction between preparing to repeat/switch tasks, not preparing to repeat/switch tasks, and preparing to not repeat/switch tasks was blurred. Experiment 1 also revealed unanticipated block order effects that may have only emerged because of the covert nature of the blockwise manipulation. The effect of explicit instructions on these block order effects may help elucidate their cause. Importantly, even with instructions at the beginning of each block, persistence- and flexibility-related processes would still need to be endogenously triggered on a trial-by-trial basis.

Method

Participants

Eighteen undergraduate students at the University of Windsor voluntarily participated in this study. They ranged in age from 18 to 49 years (median = 21). The majority (72%) were female.

Materials

Identical to Experiment 1.

Procedure

Participants were first given the same general instructions and practice blocks as in Experiment 1. When introducing the next phase of the study, participants were informed that there would be important differences between the three experimental blocks, and that these differences would be explained at the beginning of each block. For each one, participants were told the global probability of a repetition/switch trial in that block as well as how they may translate this information to local probability – "after you perform a certain task on any trial in this block, it is likely that you will be asked to perform [that same task *or* a different task] on the very next trial." Examples were provided. Finally, they were instructed to use this probability information to prepare for each upcoming trial during the brief pauses that precede them. Only after subjects were able to accurately paraphrase these instructions were they allowed to begin the block.

Participants in this experiment completed high-repetition and high-switch blocks in a counterbalanced order, before (n = 9) or after (n = 9) an equal-proportioned block.

Results

Data Screening

Boxplots of global (across all blocks) mean RTs identified one participant as an outlier. This subject's mean RT fell 2.3 standard deviations from the group mean, even though they reported a high level of motivation (10/10) to try their hardest on routine post-experiment questioning. Data for the remaining 17 participants were analyzed.

RT Data

Descriptive statistics are provided in Table 5. A 3 (block) X 2 (trial type) within-subjects ANOVA revealed no main effect for block [$F(2, 15) = 1.20$, $p = .328$, $\eta_p^2 = .138$], but a large main effect for trial type [switches > repetitions; $F(1, 16) = 54.60$, $p < .00001$, $\eta_p^2 = .773$] and a percent by trial type interaction [$F(2, 15) = 14.42$, $p = .00032$, $\eta_p^2 = .658$]. See Figure 6. Planned comparisons revealed that probable repetitions were performed faster than unpredictable [$t(16) = 3.31$, $p = .004$, Cohen's $d = .661$] and improbable repetitions [$t(16) = 2.93$, $p = .010$, Cohen's $d = .506$], with latter two not differing [$t(16) = 1.17$, $p = .258$, Cohen's $d = .244$]. There was a trend for probable switches to be performed faster than unpredictable [$t(16) = 1.88$, $p = .078$, Cohen's $d = .316$] and improbable switches [$t(16) = 2.28$, $p = .037$, Cohen's $d = .481$], with latter two not differing [$t(16) = .28$, $p = .780$, Cohen's $d = .061$]. Thus, it appears that *not preparing to* and *preparing to not* repeat/switch tasks are indistinguishable processes. (Note that effect sizes obtained for the probable versus improbable comparisons would be considered substantial by conventional interpretation, despite the marginal statistical significance associated with them.)

With block order added as a between-subjects factor (and the equal-proportioned block removed to facilitate interpretation, as in Experiment 1), a three-way block by trial type by block order split-plot ANOVA revealed no main effects for block [$F(1, 15) = .08$, $p = .784$, $\eta_p^2 = .005$] or block order [$F(1, 15) = 1.77$, $p = .203$, $\eta_p^2 = .106$], but a large main effect for trial type [switches > repetitions; $F(1, 15) = 76.27$, $p < .00001$, $\eta_p^2 = .836$]. As expected, there was a significant block by trial type interaction [$F(1, 15) = 29.92$, $p = .00007$, $\eta_p^2 = .666$]. Block order interacted with block [$F(1, 15) = 9.26$, $p = .008$, $\eta_p^2 = .382$] but not trial type [$F(1, 15) = 1.06$, $p = .320$, $\eta_p^2 = .066$]. The former indicates that participants produced relatively faster RTs for the high-repetition block when it came second and faster RTs on high-switch block when it came second. In other words, there was an overall improvement in RT from one block to the next. The three-way interaction was non-significant [$F(1, 15) = 1.26$, $p = .279$, $\eta_p^2 = .078$]. However, this is again misleading because the interactions at each level of the block order variable were driven by different (opposite) simple main effects. Post-hoc tests revealed isolated probability effects for switches among participants who completed the high-repetition block before the high-switch block [$t(8) = 3.39$, $p = .010$, Cohen's $d = .945$] and isolated probability effects for repetitions among those who completed the blocks in the reverse order [$t(7) = 4.14$, $p = .004$, Cohen's $d = .925$].

Since block order and practice are perfectly confounded, practice effects must be considered as an alternative explanation for the present replication of isolated RT differences for repetitions in one subgroup of participants and isolated RT differences for switches in the other subgroup. The practice effects hypothesis would predict decreasing repetition and switch RTs from one block to the next, regardless of the probability of a repetition/switch in those blocks (i.e., no interaction). If only probability affected RT, on the other hand, repetition trials should be performed faster in the high-repetition block compared to the high-switch block and the opposite pattern should be seen for switch trials, regardless of the administration order of these blocks. Note that these hypotheses run counter to each other in some cases but make the same prediction in other cases. Consider the group of participants who completed the high-repetition block before the high-switch block. The practice effects hypothesis predicts that repetitions and switches would be faster in the high-switch block relative to the high-repetition block. The probability effects hypothesis also predicts that switches are faster in the high-switch block, but that repetitions are *slower* in the high-switch block. The data show that switch RT is faster in the high-switch block, consistent with both hypotheses, and that repetition RT does not differ between the blocks. This latter null finding may reflect that practice and probability effects canceled each other out.

For this proposition to hold, there would have to be evidence of practice effects (unconfounded by probability effects) in the data set. A split-plot ANOVA comparing the equal-proportioned block that was completed first (before the high-switch and high-repetition blocks) and the equal-proportioned block that was completed last revealed a main effect of trial type [$F(1, 17) = 14.78$, $p = .001$, $\eta_p^2 = .465$], a null interaction [$F(1, 17) = .002$, $p = .965$, $\eta_p^2 < .001$], and most importantly, a significant between-subjects effect for order [$F(1, 17) = 12.14$, $p = .003$, $\eta_p^2 = .417$], consistent with practice effects for both repetitions and switches. Moreover, for participants who completed the equal-proportioned block first, there were within-block practice effects, as indicated by a contrast between mean RT for the first and second halves of this block [$F(1, 8) = 11.22$, $p = .010$, $\eta_p^2 = .584$]. Thus, there is strong evidence for within- and across-block practice effects over the entire course of the experiment, suggesting that an apparent absence of practice effects in certain blockwise contrasts really reflects existing practice effects countered by some other effect related to the inherent differences between blocks, i.e., probability effects.

The suggestion that practice and probability effects nullified each other also assumes that they were approximately equal in magnitude. It logically follows that when probability effects predict the same RT patterns as practice effects, the magnitude of RT difference will be approximately double that attributable to probability effects alone. Put differently, the isolated so-called "probability effects" reported above at each block order administration will overestimate the true effect of the probability manipulation by about 100%. In other words, the RT difference at each block order should be about double that for the overall data, collapsed across block order (thereby removing practice effects). Indeed, the repetition RT difference for participants who completed the high-switch block first is 150 ms, approximately double that for all participants combined (66 ms; Figure 5). Also, the switch RT difference for participants who completed the high-repetition block first is 106 ms, about double that for all participants combined (56 ms; Figure 5).

The most important finding was that probability effects were evident for both repetition and switch trials in the overall data (collapsing across counterbalanced block order to remove the influence of practice/fatigue effects), consistent with Goschke's model. Of interest, but of no theoretical relevance, when block order was examined as a between-subjects variable, practice effects were found to be substantial in our experimental paradigm, enhancing probability effects (twofold) in some contrasts and negating them in others.

Error Rates

As with the RT data, a 3 (block) X 2 (trial type) within-subjects ANOVA revealed no main effect for block [$F(2, 16) = 3.01$, $p = .078$, $\eta_p^2 = .274$], but a large main effect for trial type [$F(1, 17) = 17.05$, $p = .001$, $\eta_p^2 = .501$]. However, the interaction effect failed to reach significance, $F(2, 16) = 1.97$, $p = .172$, $\eta_p^2 = .197$. For the block by trial type by block order split-plot ANOVA, the only significant effect was trial type, $F(1, 15) = 13.29$, $p = .002$, $\eta_p^2 = .470$.

The percentage of all switch errors that were perseverative was 45.9% in the high-switch block, 59.3% in the equal-proportioned block, and 72.9% in the high-repetition block. The perseverative error rate in high-switch block was significantly lower than in the equal-proportioned block [χ^2 (1) = 3.90, $p = .048$], which was only marginally lower than the high-repetition block [χ^2 (1) = 2.83, $p = .093$]. The difference between high-switch and high-repetition blocks was large [χ^2 (1) = 12.31, $p = .00045$].

Summary of Main Findings

The methodological parameters in this experiment were identical to those in Experiment 1, except that participants were explicitly told about the probability of a switch/repetition before starting a block and instructed to use this information to prepare for each forthcoming trial. A highly similar pattern of findings emerged. Switch costs were more than twice as large (196 ms) in the high-repetition block compared to the high-switch block (74 ms), but this interaction was now driven by significant probability effects for both repetitions and switches. These simple main effects were of similar magnitude (Cohen's d ~ .5). Another important finding was that unpredictable repetition trials closely resembled improbable repetition trials, which were both performed slower than probable repetition trials. The same pattern was seen for switch trials. An examination of block order again revealed that probability effects were found only for repetition trials in one subgroup of participants, and the opposite pattern was found in the other subgroup. Follow-up analyses indicated that this was due to the presence of practice effects that were similar in magnitude to probability effects. These two effects were additive in certain conditions and cancelled each other out in other conditions. Of utmost importance, when practice effects were statistically removed, probability effects remained for repetitions and switches, as predicted. Finally, perseverative errors increased with the probability of a repetition.

General Discussion

In the present series of experiments, participants' expectancies concerning the forthcoming trial were manipulated in the random-cued version of the task-switching paradigm in order to evaluate four key assumptions of Goschke's (2000; 2003) model of the dynamic regulation of executive control. The first of these, that *task repetition can be facilitated by a preparatory state of persistence*, is highly consistent with the robust finding that probable repetition trials were performed faster than improbable repetition trials (all experiments). This effect was evident even when probability was covertly manipulated (Experiment 1). Preparation for a forthcoming repetition was also consistent with the finding that the proportion of switch errors that were of the perseverative type increased with the probability of a repetition (Experiments 1 and 3).

Before concluding full support for this assumption, bottom-up processes must be ruled out as accounting for the reduced repetition trial RT and increased rate of perseverative switch errors in the high-repetition block relative to the high-switch block. Cumulative stimulus-driven task set activation over consecutive task repetitions (i.e., repetition priming) may account for why the inappropriate set was highly activated such that subsequent repetition trials were performed very quickly, or in the case of subsequent switch trials, perseverative errors were committed. This alternative hypothesis predicts that perseverative errors would be even more frequent and probability effects on repetition RT would be even stronger in the short RCI condition relative to the long RCI condition, since the latter would allow the stimulus-driven activation to dissipate to a greater extent. This is the opposite of what was found (Experiment 2, long vs. short RCI). Moreover, RT did not tend to decrease with position in a run of repetition trials (Experiment 1), further damaging the credibility of this hypothesis. Thus, the present findings heavily implicate endogenous processes such as sustained activation of the current task set as characterizing the preparatory state of persistence. This is consistent with Dreisbach et al.'s (2002) study, in which probability of a forthcoming repetition was not confounded with the number of consecutive preceding repetition trials, and yet robust probability effects were observed.

The second assumption of Goschke's model, that *task-switching can be facilitated by a non-specific preparatory state of flexibility*, was more controversial with respect to the preexisting literature. In most prior studies, participants switched between two tasks, so that when a switch was expected, task-specific preparation could be carried out (Rogers & Monsell, 1995; Ruthruff et al., 2001; Sohn & Carlson, 2000; but see Dreisbach & Heider, in press); having just performed Task A, they would prepare for Task B. This is not relevant to the assumption under question. Having participants switch between more than two tasks is necessary to study non-specific/generic preparation for a switch – getting ready for an upcoming task switch when the particular to-be-switched-to task is unknown. Not surprisingly, there is solid evidence that task-specific preparation is superior to generic preparation (e.g., Sohn & Carlson, 2000). However, this still does not address the prediction made by Goschke's model that generic preparation is superior to no preparation, such as under conditions where the forthcoming trial type is completely random, or unpredictable (regardless of the number of tasks). Dreisbach et al. (2002) appear to be the first to have tested this prediction, and found that generic preparation is equivalent to no preparation, i.e., not facilitory. However, in the Dreisbach et al. (2002) study, multiple tasks were associated with the same stimulus features. An upcoming switch trial could have involved performing a different task based on the same stimulus feature or a different task based on a different stimulus feature. Because of the former possibility, biased processing of particular stimulus features – one tenable account for how generic preparation could be beneficial, was not advantageous. The methodology in the present study allowed it to be advantageous, and produced the finding that general preparation was better than no preparation, at least when the probability of a switch/repetition was made explicit (Experiment 3). In other words, the current findings represent the first empirical support for a generic preparatory state of flexibility that facilitates task-switching.

The finding that perseverative switch errors occurred at greater than chance frequency in the high-switch block (albeit with much less frequency than the high-repetition block) suggests that generic preparation for an upcoming switch trial, although helpful, was less than complete. That is, even when the task context promoted preparation for an upcoming switch trial during each RCI, and the RT data indicated that participants indeed did so, they were still unable to overcome the bottom-up activation of the no-longer-relevant mental set on some occasions. In other words, some degree of stimulus boundedness or "utilization behavior" appears to be normal. This is consistent with other studies of neurologically intact subjects (e.g., Robertson et al., 1997; Manly et al., 2002) that support the notion that perseverative behavior in brain-injured patients is a disorder of quantity rather than quality. This finding may also indicate that participants do not fully prepare on *every* trial, as De Jong (2000) suggests.

According to the third assumption that *the processes that underlie persistence and flexibility are antagonistic*, conditions that facilitate repetition trial performance (e.g., high repetition trial frequency) will inhibit switch trial performance and vice versa. Indeed, there was a highly significant interaction between trial type and probability of a repetition/switch that was supported by opposing simple main effects for both repetitions and switches, at least in Experiment 3. However, improbable repetitions/switches did not differ from unpredictable repetitions/switches (Experiments 1 & 3), indicating that preparing to perform one trial type does impair performance of the other trial type beyond baseline (i.e., an unprepared state). In other words, performance on repetition trials was not impeded by expectations that a forthcoming repetition trial is unlikely relative to no expectations. The same was true for switch trials.

At first glance, this finding appears incompatible with the several studies reported above that demonstrate a "trade-off" between persistence and flexibility. However, closer scrutiny revealed that these so-called trade-offs were between subjects or within-subjects between different conditions (repetition/persistence performance measured in one condition and switching/flexibility measured in another). These prior findings therefore do not necessarily conflict with the present finding of no trade-off within-subjects in the same condition.

The implication of this failure to support Goschke's "antagonistic" assumption is that persistence- and flexibility-related processes are independent and thus suggests that they cannot be controlled by a single mechanism, or neural system. Dreisbach and Goschke (2004) proposed a global mechanism that modulates both persistence and flexibility, such as adjustments to "the threshold that must be exceeded by new information to gain access to working memory" (pg. 351). A high threshold would shield the current mental set from distracting information, supporting persistence, whereas a low threshold would promote background monitoring of potentially relevant information, supporting flexibility. The present findings are inconsistent with this or any other unitary mechanism model.

Single-mechanism models of persistence-flexibility regulation parsimoniously account for why many neurological patients exhibit both perseveration and distractibility. However, they are harder to reconcile with reports of double dissociations between various patient groups on measures of persistence and flexibility. For example, Stuss and colleagues (2000) demonstrated that patients with inferior medial frontal lobe lesions committed an inordinate number of set-loss errors but no more perseverative errors than non-frontal lesioned controls on the WCST. Patients with superior medial frontal lesions exhibited the opposite pattern. In another study, Yogev, Hadar, Gutman, and Sirota (2003) found that relative to controls, schizophrenics with mostly negative symptoms tended to commit more perseverative errors whereas those with mostly positive symptoms evidenced over-switching in a modified WCST paradigm.

Another corollary of demonstrating that persistence- and flexibility-related processes operate independently is that the mechanisms that underlie them must not involve inhibition of task-irrelevant information. With respect to persistence, sustained activation of the current set is proposed as the mechanism for facilitation of probable repetition trial performance. Note that this differs from distracter resistance only (Dreisbach & Goschke, 2004) and sustained activation plus distracter resistance (Miller & Cohen, 2001) models of persistence behavior that presume inhibitory processes. As for flexibility, it has been previously demonstrated that backward inhibition of the no-longer-appropriate set only occurs with activation of a new set (Hubner et al., 2003; Mayr & Keele, 2000), i.e., not with generic preparation for a switch. In the present study, preparation for a probable switch trial could not have involved activation of a new (now-relevant) set because the particular upcoming task (and therefore the relevant set, or stimulus-response mapping) was not known.[6] It could also not involve simultaneous partial activation of multiple (potentially-relevant) sets because this would cause crosstalk (Ruthruff et al., 2001). Biased processing of (relatively) novel stimulus features (e.g., Dreisbach & Goschke, 2004) or complex conjunctives of potentially-relevant stimulus features (e.g., color and shape; Desimone & Duncan, 1995) is possible however,

[6] Activation of the new set is likely at play in task-specific switch preparation, though, and probably explains why this type of preparation is superior to generic switch preparation.

and may facilitate retrieval of a new mental set. In other words, flexibility may involve biased processing of stimulus features that facilitate retrieval of new mental sets rather than direct control of mental sets themselves. This may represent a viable explanation for why generic switch preparation is beneficial (and why it did not appear to be in the Dreisbach et al. (2002) study).

Consistent with this idea, a recent study using behavioral and neuroimaging probes provided evidence that top-down input involves amplification of task-relevant features and no accompanying inhibition of task-irrelevant information (Egner & Hirsh, 2005). In a Stroop-like paradigm with famous names and faces, participants responded faster to incongruent trials when they were preceded by incongruent trials compared to when they were preceded by congruent trials. They found simultaneously greater fMRI activation of the fusiform face area of the visual cortex in the former trial type compared to the latter, when the faces were targets (and the names were distracters). Activation of this same region was remained at baseline when the faces were distracters, i.e., no cortical inhibition of task-irrelevant features was observed. They concluded that "target-feature enhancement constitutes the main selection mechanism when attention regulation is driven endogenously as to optimize performance" (pg. 1788), and proposed the neural mechanism for this as frontal signals that amplify the pre-stimulus baseline activity in the cortical region involved in processing a particular stimulus feature, spatial location, or object. However, as the authors point out, it remains to be demonstrated whether target-feature enhancement via cortical amplification applies similarly to preparatory (top-down) processes driven by expectancies regarding the upcoming stimulus (as in the present study) and that triggered by conflict (as in their study).

With regard to the fourth assumption, *the balance between persistence and flexibility processes is regulated in response to changing task context,* it no longer makes sense to discuss a "balance" between them because they appear to independent rather than antagonistic. There is, however, solid evidence that processes supporting persistence and flexibility are dynamically regulated. In the present study, changing task context was operationalized as probability of switch/repetition between blocks. Participants' expectancies regarding the upcoming trial type (repetition vs. switch) were influenced by subjective impressions of the relative frequency of switch/repetition trials (trial history in Experiments 1 and 2, and trial history + instructions in Experiment 3). Conditions that promoted expectations that a forthcoming trial was likely to require a task repetition led to facilitation of performance on repetition trials, while the opposite conditions facilitated performance on switch trials. In other words, the "persistence-flexibility dilemma" is best conceptualized as allocating top-down resources to support task continuation versus task shifting in order to optimize performance, given the task demands. The cost of misallocating top-down input seems to be essentially having no (useful) top-down input (i.e., a baseline unprepared state), rather than having to recover from a disadvantaged, or counterproductive state, as Goschke's original model proposed.

In summary, the present data support several assumptions of Goschke's model but suggest revision of others, and therefore advance our understanding of executive control. Specifically, they confirmed that task repetition can be facilitated by top-down set-maintenance processes, but also suggested that generic (non-task specific) preparation can facilitate task-switching, and that endogenous persistence- and flexibility-related preparatory processes are independently (rather than antagonistically) regulated in response to changes in the task context. This helps to explicate the nature of top-down contributions to attention and fits nicely with existing models of emergent executive control. Norman and Shallice's (1986) supervisory attention theory states that top-down input is triggered when stimulus-driven behavior is inappropriate or insufficient. Botnivick et al.'s (2001) conflict monitoring hypothesis explains how this top-down input is triggered. The present revised-model takes this one step further by specifying that top-down input involves sustained activation of the current set if persistence is required or biased processing of novel or potentially task-relevant stimulus features to facilitate retrieval of new set if flexibility is required. These models can therefore be seen as complementary. The neurobiologically plausible cognitive mechanisms for promoting persistence and flexibility proposed here are not entirely novel, but rather a mere extension of Miller and Cohen's (2001) model of relative activation of task-relevant processing pathways and Desimone & Duncan's (1995) biased competition model of selective visual attention. It should now be clear that executive control emerges from the interaction of distributed cognitive systems, and therefore no homunculus is needed. It should also be highlighted that this is not a global theory of prefrontal cortex function but merely one important function that relies heavily on prefrontal regions and their posterior cortical and subcortical connections. The prefrontal cortex is involved in cognitive functions that go beyond controlled attention (Miller & Cummings, 2007).

Updated Literature Review

Following data collection for the present experiments, I became aware of two highly relevant research studies. Dreisbach and Haider (in press) also manipulated the percentage of repetition/switch trials in a block using two levels, 75/25 and 25/75. They informed subjects of this manipulation and encouraged them to make use of it to optimize their performance, either at the beginning of each block (global probability condition) or in between every trial (local probability condition). In their paradigm, subjects switched back and forth between two tasks – deciding whether digits (1 to 9) were odd/even or smaller/larger than five. The cue indicating which task to perform was presented simultaneously with the target stimulus. The RCI was 1400 ms. The researchers found that overall RT was faster in the high-repetition block condition compared to the high-switch block condition and interpreted this as reflecting higher demands of the latter. More importantly, probable repetitions were performed faster than improbable repetitions in both conditions. Somewhat in contrast, probable switches were performed faster than improbable repetitions only in the local probability condition[7], and to a lesser degree than the repetition RT differences. This mirrors the disparity between repetition vs. switch probability effects in the current study. The authors did not offer an account for this disparity and nevertheless concluded that both probable repetition and switch trials can be prepared for, and that their findings are consistent with Goschke's model of dynamic adjustment of cognitive control. Since their method involved two tasks, preparation for switches was task-specific, whereas it was generic in our study. Thus, their finding that probable switches were performed faster than improbable switches (at least in the local probability condition) does not address the assumption of Goschke's model. The present study also added to the Dreisbach & Haider (in press) by showing that probability effects appear even when participants are given no probability information, but rather have to

[7] Although not addressed by the authors, it is surprising that they found only inconsistent evidence of beneficial preparation for probable switches when the identity of the forthcoming task was known. This is at odds with several previous studies (e.g., Dreisbach et al., 2002; Ruthruff et al., 2001; Sohn & Carlson, 2000).

deduce it themselves. In fact, according to the manipulation check results, only about 30-40% of participants were conscious of the differing probability, consistent with the finding that people have poor subjective awareness of their preparatory processes in task-switching (Meiren, Hommel, Bibi, & Lev, 2002). Because the present study included a control condition (equal-proportioned block), it was also able to clarify that the probability effects were driven by RT-benefits of preparing for a repetition/switch rather than RT-costs of preparing not to perform a repetition/switch.

Another very recent and relevant study was conducted by Monsell and Mizon (in press, Experiment 4). They manipulated the probability of a task switch in the same manner as the present study, by varying the proportion of switch trials in a block (25, 50, and 75), but between-subjects. They employed bivalent stimuli that participants performed "shape" and "color" tasks on, with two different cues for each task. The authors held the RCI constant at 1650 ms and the CSI was either 140 ms or 790 ms. At a short CSI (like the present study), Monsell and Mizon found large switch costs (switch RT - repetition RT; ~200 ms) when switches were unlikely, a smaller but statistically robust switch cost (~100 ms) when switches and repetitions were equally likely, and negligible switch costs when switches were likely (~20 ms). This is highly consistent with our finding that switch costs were large in the high-repetition block and virtually absent in the high-switch block. Unfortunately, repetition and switch trials were not entered as separate dependent variables in the Monsell and Mizon (in press) study, so their results do not clarify whether the reduction in switch costs with increasing probability of a switch trial was due to a loss of repetition benefit or gains in preparing for a switch, or both. Granted, this was not the aim of their study.

Limitations

Despite the (albeit somewhat mixed) prior research suggesting that practice effects would be minimal, block order was a significant between-subjects factor in all three experiments. In the subgroup who completed the high-switch block first showed reliable RT differences for repetitions but not switches, whereas the subgroup who completed the blocks in the reverse order showed reliable RT differences only for switches. As discussed above, this pattern can be explained by a combination of practice effects and probability effects of a similar magnitude, such that they enhance RT differences when in agreement and nullify RT differences when in contrast. Clearly, the results cannot be attributable solely to practice effects, otherwise there would be only null effects in the overall data and no interaction effect at each block order. What we found were effects for both repetitions and switches in the overall data (with the influence of practice/fatigue effects removed; Experiment 3) and interaction effects at each block order, although these were driven by a main effect for one variable and not the other, suggesting at least some influence of practice. Interestingly, although not reported in their article, the Dreisbach & Haider (in press) observed a similar pattern of block order effects to the present study, though not in both subgroups of participants and not to the same degree (Dreisbach, personal communication, 2006).

The differences between Experiments 1 and 3 were mainly in magnitude rather than pattern. It seems likely that overtly instructing participants about the probability information made a greater number of them aware of these differences[8] and strengthened

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[8] Differential awareness alone does not seem account for the between experiment differences. When a manipulation check variable (noticed vs. failed to notice varying repetition/switch trial frequency) was added as a between-subjects factor to the 3 (block) x 2 (trial type) repeated-measures ANOVA in Experiment 1, it did not modify the probability effect [$F(2, 16) = .703, p = .510, \eta_p^2 = .081$], the trial type

their confidence in/commitment to preparing for the repetition/switch trial disproportions. The only appreciable interexperiment discrepancy was that the probability effect for switches was minimal and non-significant in Experiment 1 (in the context of a substantial probability effect for repetitions), whereas it was substantial in Experiment 3. The highly similar magnitude of the repetition and switch probability effect sizes in Experiment 3 suggests that insufficient statistical power in Experiment 1 cannot explain the discrepancy. Of note, the results of a recent study (Dreisbach & Haider, in press, discussed above) replicated my finding that switch trial performance is less sensitive to manipulations. They demonstrated probability effects for switches when the probability information was provide on a trial-by-trial basis but not when it was merely provided at the beginning of the block, whereas probability effects for repetitions were seen in both cases. These discrepancies in both the present and Dreisbach & Haider's (in press) study may reflect that preparing to switch tasks is entirely an endogenous process and is facilitated by greater environmental support. In contrast, preparing to repeat tasks is to some extent promoted by exogenous forces (priming) but can also be enhanced by endogenous input.

The three tasks used in this study were intended to be roughly equivalent in difficulty. Unexpectedly, classifying objects based on a shape was more difficult than quantity or color judgments. This may be an inherent flaw in the design of the WCST stimuli, where the three figures are arranged in a triangular form and the four figures are arranged in a square. This may cause added confusion because, for example, matching a target stimulus (card from the deck) containing triangles based on shape could be

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effect [$F(1, 17) = 1.741$, $p = .204$, $\eta_p^2 = .093$], or the block by trial type interaction [$F(2, 16) = 2.886$, $p = .085$, $\eta_p^2 = .265$; unexpectedly, the interaction was marginally stronger in the group that failed the manipulation check].

sensibly paired with the reference stimulus (key card) containing triangles, but also the reference stimulus that contains different shapes but resembles a triangle at the global perceptual level. The developmental sequence of perceptual dimensions may be an alternative explanation (cf., Odom & Guzman, 1972). Regardless, the three tasks were counterbalanced across blocks and trial types within blocks, and so should not have systematically altered the main analyses.

Future Research & Applications

To evaluate the robustness of the present findings, it would be helpful to test the effects of parameter manipulations (60:40/40:60 ratios in the high-switch and high-repetition blocks). The four main assumptions of Goschke's model should also be re-evaluated using a different experimental paradigm, to rule out methodologic specificity. To better understand why generic switch preparation is beneficial, future studies should design experiments that contrast the hypotheses that biased processing is of novel versus potentially task-relevant stimulus features. The present data cannot untangle these possibilities.

Functional neuroimaging studies would help elucidate the neuroanatomical underpinnings of persistence and flexibility related-processes. There is converging evidence that traditional versions of the task-switching paradigm activate a complex network that includes lateral prefrontal areas as well as non-frontal regions (Brass & von Cramen, 2002; Dove, Pollmann, Schubert, Wiggins, & von Cramon, 2000; Kimberg, Aguirre, & D'Esposito, 2000). Interestingly, when the upcoming task is not explicitly cued (i.e., environmental support is reduced), as in the present study, greater medial frontal activation is seen (Forstmann, Brass, Koch, & von Cramon, 2005). This corresponds with neuroimaging research using conflict resolution paradigms (e.g., Stroop), in which medial frontal (including anterior cingulate cortex) activation is thought to represent signaling for greater endogenous control by dorsolateral prefrontal regions (MacDonald, Cohen, Stenger, & Carter, 2000). In the present study, the task context changed, demanding dynamic adjustment of executive control. This version of the task-switching paradigm can be predicted to rely on an interaction between medial and dorsolateral frontal systems to a greater degree.

According to Goschke's theory, the cognitive hallmark of damage to the frontal systems is impaired context-sensitive dynamic regulation of persistence and flexibility such that patients may inappropriately maintain set when they should be switching and at other times (perhaps within the same test administration), be excessively distractible when sustained focus is required. In other words, it is the maladaptation to a changing environment rather than impaired set-maintenance or set-shifting per se. Based on this premise, the task-switching paradigm developed in the present study may be particularly sensitive to frontal-subcortical dysfunction. This claim could be evaluated by administering the task-switching paradigm developed for the present study to patients with various clinical disorders that involve the frontal systems such as traumatic brain injury, frontotemporal dementia, schizophrenia, and Attention-Deficit/Hyperactivity Disorder.

Insofar as this holds true, the paradigm may be useful in the clinical assessment of neurological and psychiatric patients. The factor-purity of this paradigm, relative to traditional measures of executive control such as the WCST, might help to better characterize the nature of cognitive impairment following frontal systems damage and predict real-world functioning. Because of the substantial practice effects across blocks, adaptation would be needed. Either a long block with progressively increasing and decreasing probabilities of switch/repetition trial, or an ABABAB blocked design might be a solution. In the same vein of modern approaches to the assessment of executive functioning (e.g., the Delis-Kaplan Executive Function Systems battery; Delis, Kaplan, & Kramer, 2001), it would probably be helpful to include control task(s) in order to parse out the non-executive functioning components of task-repetition/switching performance (e.g., simple reaction time).

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Table 1. Trial type by response time data for equal-proportioned block in Experiment 1.

Trial type	Response type	RT	Error Rate
Repetition	Repetition	704 (128)	7.9 (4.6)
	Switch	812 (102)	5.1 (4.7)
Switch	Repetition	952 (235)	9.9 (8.3)
	Switch	875 (138)	6.9 (6.4)

Table 2. Task type by trial type data for equal-proportioned block in Experiment 1.

Task type	Trial type	RT	Error Rate
Color	Rep	760 (104)	3.4 (1.2)
	Switch	806 (124)	9.0 (2.6)
Shape	Rep	942 (146)	9.4 (2.2)
	Switch	1027 (171)	7.8 (2.8)
Number	Rep	718 (118)	4.4 (2.6)
	Switch	861 (159)	9.3 (2.0)

Table 3. Block by trial type data for Experiment 1.

Block	Trial type	RT	Error Rate
High-switch	Rep	806 (155)	4.3 (4.9)
	Switch	880 (185)	8.0 (5.2)
Equal-proportioned	Rep	785 (104)	5.8 (4.1)
	Switch	889 (158)	7.7 (6.2)
High-repetition	Rep	736 (99)	4.8 (2.6)
	Switch	918 (137)	7.1 (6.0)

Table 4. Block by trial type data for Experiment 2.

RCI	Repetition:switch proportion	Trial type	RT	Error Rate
1000 ms	High-switch	Rep	818 (112)	4.6 (4.1)
		Switch	910 (116)	9.5 (7.4)
	High-repetition	Rep	741 (93)	6.4 (3.9)
		Switch	912 (149)	6.3 (4.6)
100 ms	High-switch	Rep	913 (156)	8.5 (7.6)
		Switch	1049 (200)	10.1 (7.5)
	High-repetition	Rep	839 (110)	7.1 (4.5)
		Switch	1030 (165)	10.2 (7.1)

Table 5. Block by trial type data for Experiment 3.

Block	Trial type	RT	Error Rate
High-switch	Rep	845 (127)	5.0 (5.0)
	Switch	919 (150)	8.2 (4.1)
Equal-proportioned	Rep	883 (179)	5.7 (3.1)
	Switch	972 (236)	7.1 (4.5)
High-repetition	Rep	789 (94)	6.4 (2.9)
	Switch	985 (160)	9.1 (7.6)

Figure 1. Pictorial display of stimulus-response mappings with a sample stimulus. This stimulus would be correctly matched to the green square (the'C'key) if the color task is cued, with the two blue plus signs ('V') if the number task is cued, and with the four yellow triangles ('N') if the shape task is cued.

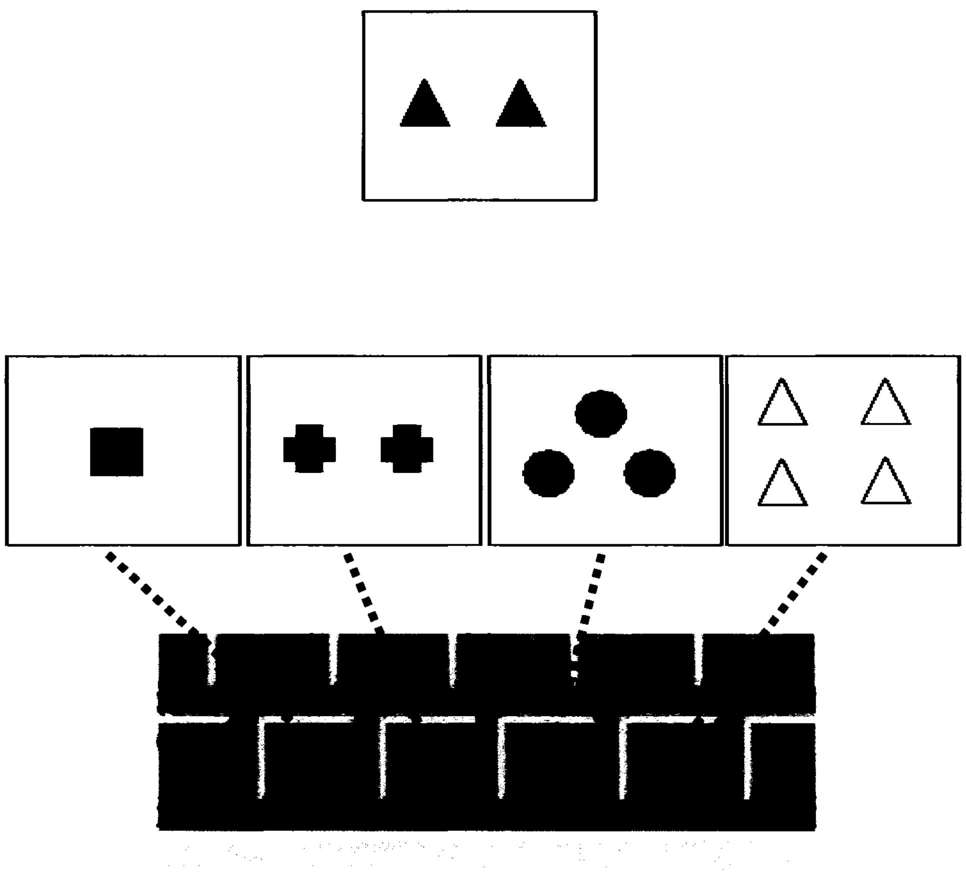


Figure 2. Illustration of event sequence for a sample trial.

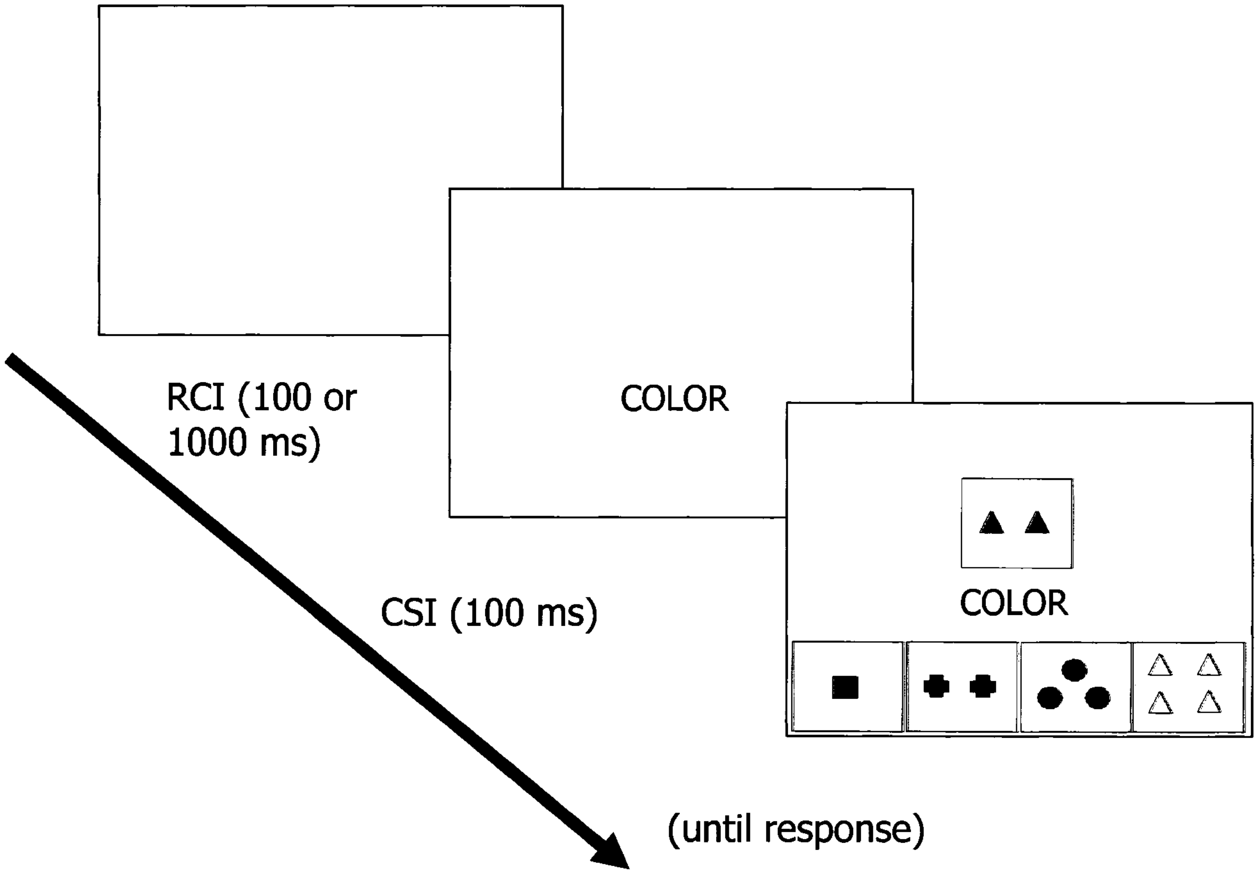


Figure 3. Distributions of response time performance by trial number for the mixed practice block.

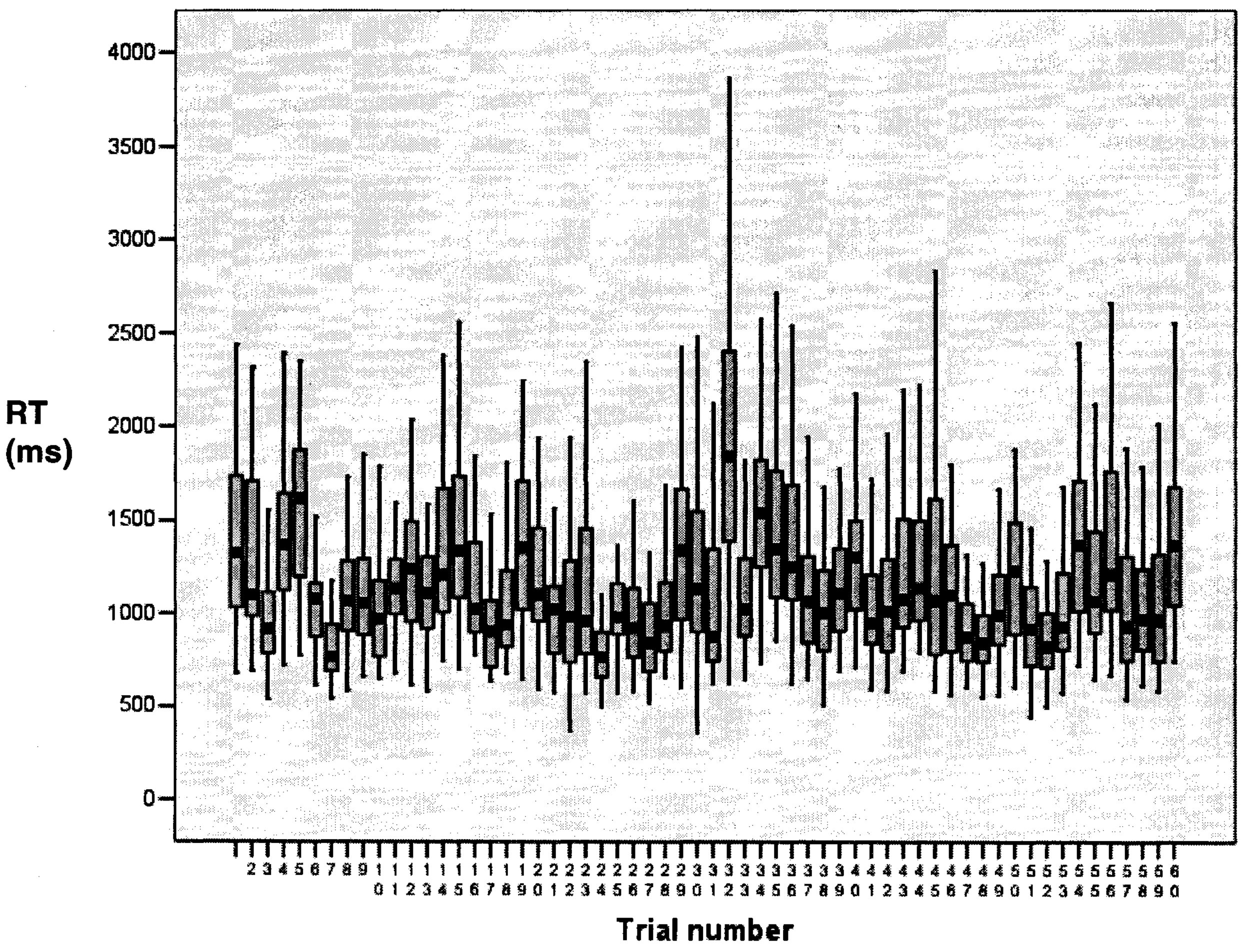


Figure 4a. Mean response times for various task recency values.

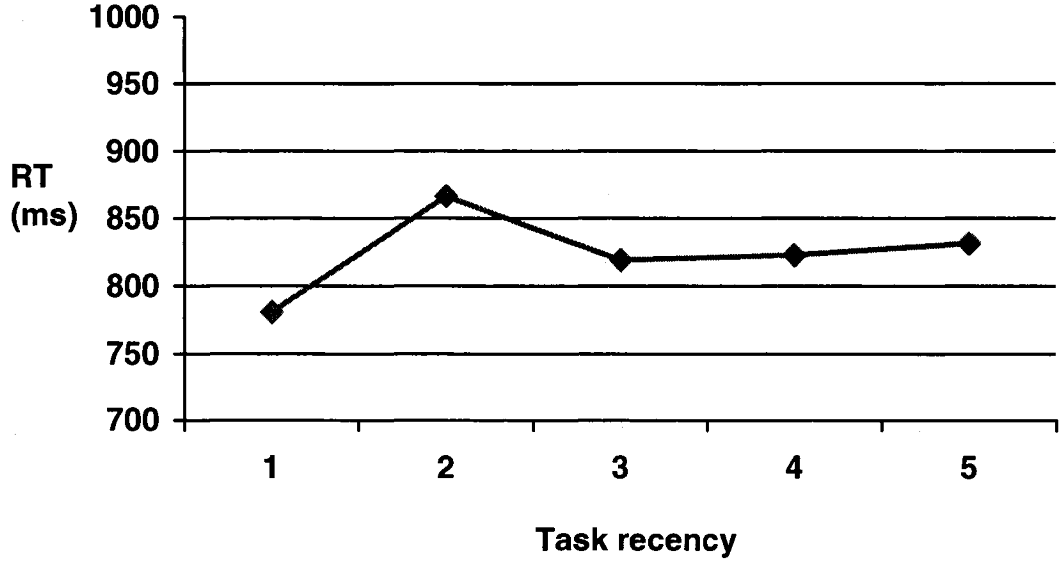


Figure 4b. Mean error rates for various task recency values.

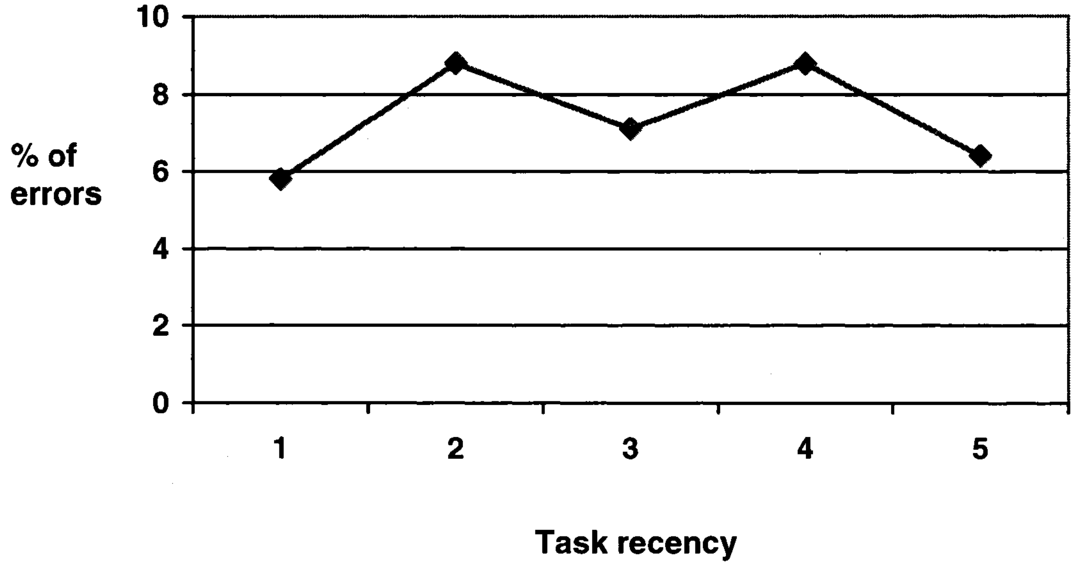


Figure 5. Overall response time data for Experiment 1.

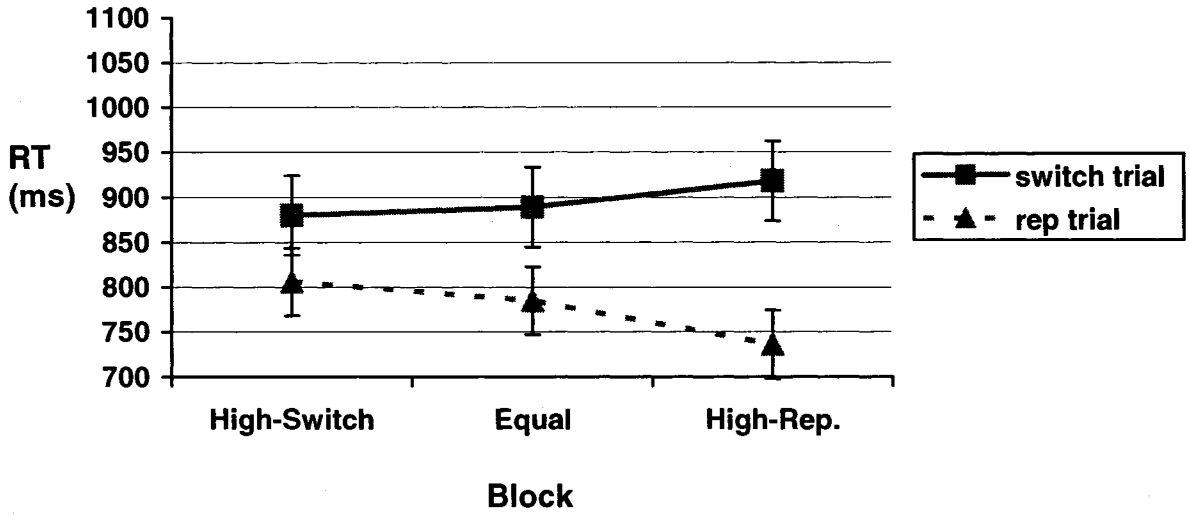
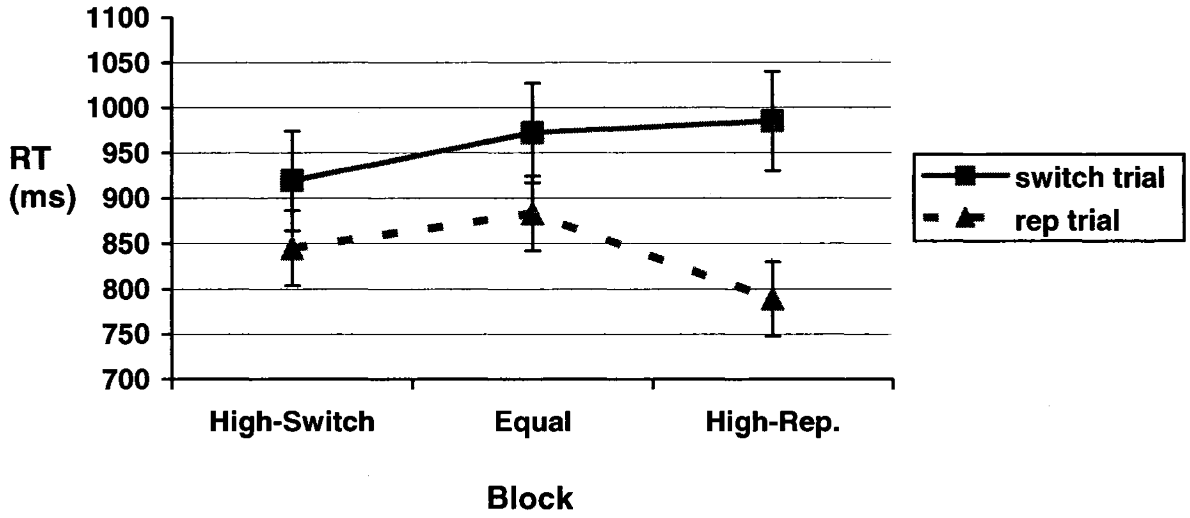


Figure 6. Overall response time data for Exp. 3



Appendix A

Manipulation check questions:

1a. What was different about the four different parts ("blocks") of this experiment?

1b. In the block with the least switch trials, what percentage of the trials were switch trials?

1c. In the block with most switch trials, what percentage of the trials were switch trials?

1d. Did the difference in the percentage of switch trials change your response strategy in each of the blocks? How so?

2. How motivated were you to try your hardest, on a scale from one to ten?

3a. How concerned were you with answering as fast as you could, on a scale from one to ten?

3b. How concerned were you with answering correctly, on a scale from one to ten?

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